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Certain Aspects of Henry's Experiments on Electromagnetic Induction: PRESIDENT JOSEPH S. AMES 87

The American Association for the Advancement of Science:
The Relationships of the Natural Sciences and the Social Sciences in Agricultural Education in the United States: DEAN W. C. COFFEY 92

Scientific Events:
The Importation of Scientific Specimens and Apparatus into Great Britain; The University of Porto Rico and the U. S. Department of Agriculture; To Guadalupe Island; Professorships at Yale University in Memory of Gibbs and Sumner; Award of the First Richards Gold Medal to Professor Arthur A. Noyes 97

Scientific Notes and News 99

Discussion:
Definition of a Mathematical Group: PROFESSOR G. A. MILLER. *Authors' Abstracts:* MARCUS I. GOLDMAN. *The Distribution of State Agricultural Experiment Station Bulletins to Foreign Countries:* PROFESSOR J. G. LEACH, PROFESSOR H. MACY and PROFESSOR C. H. BAILEY. *Auditory Action Currents:* DR. GEORGE KREEZER and HANS DARGE. *Thallium Poisoning and Soil Fertility:* PROFESSOR S. C. BROOKS. *Essential Fatty Acids and Goiter Producing Substances:* PROFESSOR F. E. CHIDESTER. *The Discovery of the Oestrus Cycle in Man and Other Mammals:* DR. WALTER LANDAUER. *Chromosome Numbers in Ulmus:* DR. RUTH I. WALKER 102

Scientific Apparatus and Laboratory Methods:
Photomicrography with a Vest Pocket Camera: DR. OSCAR W. RICHARDS. *A New Technique for Obtaining Oospores of the Hop Downy Mildew by Inoculating Cotyledons:* DR. WALTER JONES 107

Special Articles:
On the Variation of the Oxygen Content of Cultural Solutions: DR. W. A. CANNON. *A Diaporthe Canker of American Elm:* BLYTHE G. RICHMOND. *A New Bacterial Disease of Pears:* FELICIANO M. CLARA. *The Production of Mucification of the Vaginal Epithelium of Rodents by the Oestrus Hormone:* DR. ROLAND K. MEYER and WILLARD M. ALLEN 108

Science News 8

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CERTAIN ASPECTS OF HENRY'S EXPERIMENTS ON ELECTROMAGNETIC INDUCTION[1]

By President JOSEPH S. AMES

THE JOHNS HOPKINS UNIVERSITY

I REGARD it as a great honor to be invited to give the first of a series of lectures before the Washington Philosophical Society, to be known as the Joseph Henry lectures, established in honor of the founder and the first president of the society.

I think it is undoubtedly proper for me to choose as the topic of this first lecture one which is related to the life of Joseph Henry, partly because of the association of his name with the lectureship, but also because this year is recognized as the one hundredth anniversary of the discovery of the phenomena of electromagnetic induction, a discovery with which the name of Joseph Henry will always be associated.

This discovery of electromagnetic induction marked the beginning of the modern era of electricity and in fact of the modern era of physics, and it is therefore most fitting that a celebration of the centennial anniversary of the discovery should take place. Last month such a celebration was held in London at the Royal Institution, to commemorate the part Michael Faraday played in the discovery. Although he was anticipated in this by Joseph Henry, so far as both mutual induction and self-induction are concerned, Faraday will always be regarded, properly, I think, as their real discoverer because he was the first to publish the results of his investigations and pointed out at the time of his first announcement the possibility of making practical application of them. Joseph Henry, himself, although he deeply regretted the fact that he had delayed publication of his investigations,

[1] An address delivered before the Philosophical Society of Washington on October 24, 1931.

always gave full credit to Faraday and was scrupulously careful to refer to him as the discoverer. The newspapers, magazines and scientific periodicals have called attention repeatedly during the past two months to the work of Faraday and to his greatness as an experimenter and as a philosopher of nature. Rather scant notice has been given to the work of Joseph Henry, one writer saying that he "probably anticipated" Faraday; and my real reason for selecting the topic I have for this evening is my desire to impress upon all of you who listen to me the essential facts of Henry's great discoveries. Certainly whenever an American physicist hears the words, induced currents, the first thought that should come to his mind is "Joseph Henry discovered these."

It will indeed be superfluous for me to give a sketch of the life of Henry or to refer in detail to the long series of his brilliant investigations in the varied fields of physics or to his great contributions to the scientific life of this country. Any one who is interested in these matters should read the address by Professor William B. Taylor which was read before this society fifty-three years ago this month and which was published by the government in a volume devoted to the life and scientific work of Henry, and especially the excellent article by Professor Magie in the October number of *Reviews of Modern Physics*.

I intend to confine myself this evening to the question of induced electric currents and, even more narrowly, to the original experiments of Henry, calling attention particularly to the dates at which his discoveries were made. I must begin, however, by making as the background of my paper a statement concerning the scientific knowledge available at the time Henry began his experiments and also a description of Henry's environment, so that you may understand more clearly the stimulus which animated him and the difficulties under which he labored.

In 1819 Oersted made the discovery that an electric current flowing in a conductor exerted a force upon a magnet, and this great event was, of course, followed at once by investigations all over Europe. In the following year Schweigger devised his multiplier, consisting of an arrangement by which a magnetic needle came under the influence of several turns of wire, and thus perfected a rather sensitive instrument for the detection of an electric current. In this same year, 1820, Arago and Davy discovered independently that a steel needle placed axially inside a helix of wire became magnetized when an electric current was passed through the helix. In this experiment of Arago's the wire was uncovered and was wrapped in a loose helix around a glass tube, the needle being placed inside this tube. In the same year also Ampère began his brilliant series of studies on the action of currents on magnets and of currents on currents, resulting in the discovery of phenomena which form the basis of electrodynamics. In these experiments Ampère used single turns of bare wire. In 1825 William Sturgeon, of Woolwich, England, really developed the electromagnet by winding uncovered wire around an iron bar, which was insulated from the wire, the iron bar itself being bent into the shape of a horseshoe so that an armature could be placed across its two ends. In this electromagnet Sturgeon used eighteen turns, loosely coiled. These were the essential facts concerning the relations between electricity and magnetism known to the scientific world at the time Henry began his studies.

In 1826 Joseph Henry was elected professor of mathematics and of natural philosophy at the Albany Academy. This was one of the outstanding high schools of the State of New York, and the work done there compared favorably with that characteristic of some of the colleges of that day. He was twenty-seven years old at the time of his appointment, and his interests were already centered in the study of natural philosophy, his attention having been called to the subject when he was fifteen years old by the chance reading of a book which he had found left on a table by a man boarding with his mother. This book, Dr. G. Gregory's "Lectures on Experimental Philosophy, Astronomy and Chemistry," made a great impression upon the boy's mind, and it is recorded that the owner gave it to him and that he kept it with him throughout his entire life. It aroused his intellectual curiosity and inspired him with a desire to answer questions dealing with nature. He set to work at once to perfect himself for this life of investigation, and after some years of study, teaching and tutoring, during which time he presented several papers before the local scientific society, the Albany Institute, he was chosen unanimously to fill the position I have mentioned. Albany at that time was a small city practically on the frontier, remote from centers of scholarship or of study, but the Albany Academy was a very worthy institution. It occupied a large building in the center of the city, consisting of classrooms and one good-sized hall in its center, used for general purposes. There was no laboratory, of course, as such, and no apparatus of any kind; consequently, Henry had to do everything with his own hands except so far as he could call upon a blacksmith to help. Schools in those days were rather serious-minded institutions and Henry was kept fully occupied with his classes as long as the school was operating, which was for practically ten months of the year. The sessions began the first of September and during all the time Henry was professor in the academy the only opportunity he had for experimental investigations was during the summer vacations, and practically for only one month, the last part of

July and the first part of August. When the vacation began Henry would set up his apparatus in the main hall of the building and he could work with it until in August the time came for the building to be cleaned and gotten ready for the classes which met, as I have said, the first of September.

I must ask you to consider this situation and to contrast it with that of the investigators in Europe, especially of Faraday. Here was a man evidently full of ideas, evidently eager to add to knowledge and to test his theories by experiments; but he had no apparatus, he had no means to buy any even if it had been available and he had at his disposal only a few weeks of the year in which he could devote himself to what was really the purpose of his life. When one considers these circumstances it is extraordinary indeed that Henry should have been able to advance as far as he did.

Henry read with eagerness everything that was published in regard to the connection between electricity and magnetism and as soon as he became a professor at the Albany Academy he began to make plans for the continuation of the experimental work of which he had read. The first thing he planned to do, naturally, was to repeat the work of others so as to convince himself of its accuracy and to become more familiar with the phenomena. In thinking over the experiments done by others he believed that he could increase the sensitiveness of the apparatus and could magnify the forces by an extremely simple device, which apparently had not occurred to any one else. This was to use coils of insulated wire in place of single turns, and in October, 1827, he demonstrated before the Albany Institute the various experiments of Ampère and others, using coils and thus magnifying the effect greatly. In order to superimpose one layer of the coil upon another layer, thus forming a bobbin, it was obviously necessary to insulate the wire itself, which he did by wrapping the wire "with silk," as he says. I have heard from the family that "the silk" used by Henry in making some of his first coils was really a series of ribbons of silk obtained by the sacrifice on the part of his wife of her white silk petticoat. An electromagnet made by Joseph Henry and used in his experiments is still in existence, being treasured in the museum at Princeton University, and one can still see the white silk ribbons used in its construction, so I believe the story I have heard is true.

One can hardly overestimate the importance of this device of Henry's. No one up to this time had thought apparently of using insulated wire coiled in layers, and immediately after Henry described his apparatus all the investigators of Europe adopted the idea. Faraday, in his original apparatus, used when he made his discovery of electromagnetic induction, wound bare wire on an insulated iron ring, the separate coils being kept apart by winding between them a long twine, the separate layers being kept apart by pieces of non-conducting cloth. One can not tell by reading Faraday's papers whether this idea was original with him or whether it was adopted after having seen Henry's published paper. As a matter of fact, in no one of Faraday's papers is there any reference to the experimental work of Joseph Henry.

Henry's first application of this new principle of coiling long wires into a number of layers was in the construction of a small electromagnet which he exhibited before the Albany Academy in June, 1828; and, having found how successful it was, he made another more powerful one the following year, which he exhibited in March, 1829. He then had a new idea in regard to the winding of magnets and in the latter part of the same year made one in which he had two independent windings over the whole length of the iron core, so that he could join these two in parallel. Then, in order to make a magnet which could be used for many experimental purposes, in August, 1830, he wound one in a distinctly novel way, producing a magnet by far the most powerful then in existence. He wound the core of this magnet with nine separate coils, each coil occupying a space of about two inches. The terminals of each coil extended out from the side so that he was able to join the nine coils either in series or in parallel. This offered him a great variety of experimental possibilities. He investigated in connection with this magnet the effect of using a number of voltaic cells arranged in parallel or in series, and he emphasized the effect of having the cells when in series joined with the coils of his magnet also in series and similarly the effect of having either one cell or all the cells in parallel joined with the coils of his magnet when these were in parallel. He made a study of what we would to-day call Ohm's law so far as the effect of the internal resistance of batteries is concerned and also performed many other experiments, some of which I will refer to later. All this was in August, 1830. In November of the same year he saw in Brewster's *Edinborough Journal* an account of some work done by the Dutch physicist, Moll, in regard to electromagnets and thought it best to write an account of his work up to that time upon the same subject. This he did in November and submitted it for publication in *Silliman's Journal*. It was published in January, 1831, and was Henry's first scientific publication in a recognized scientific journal. It was characteristic of Henry for many years not to publish his results as soon as he had obtained them, but rather to wait until he had tested and investigated the various ideas which were in his mind on any one

subject so that he would be convinced that he had exhausted the possibilities of that particular line of thought. This quality of his mind resulted naturally in the fact that so far as publication was concerned he was anticipated often, and he was in several cases persuaded by his friends to write brief accounts of what he had done after the publication by others of their work.

I refer above to experiments which Henry made with his final form of magnet, and these he did not include in his paper in *Silliman's Journal,* which was confined simply to a description of his magnets. His reason for not publishing some of these experiments, which were made in August, 1830, was because he intended to continue these further, and during August, 1831, he was very busy preparing some new apparatus, one portion of which was a large reel containing a mile of wire and another was a much more powerful electromagnet. In a letter of November 6, 1831, he says that he had been making this apparatus "for some contemplated experiments in the identity of electricity and magnetism." In another letter he says that he had to stop his work on this new apparatus because the room in which he was working was wanted for the classes of the academy. It is clear, therefore, that in August, 1831, he had been occupied with the construction of apparatus with the idea of continuing some of the work which he had begun in August, 1830, and which he had not thought to be sufficiently advanced to justify publication.

Immediately after the discovery of the fact that an electric current through a coil of wire would magnetize a piece of iron and in fact that a coil of wire carrying an electric current had magnetic properties, many investigators felt that there should be some way by which an electric current could be produced by means of a magnet, and experiments of various kinds were tried. Among others, Michael Faraday, of the Royal Institution, London, occupied himself with the problem for some years and on August 29, 1831, began a series of experiments which finally solved the problem. He reported the essential features of his experiments to the Royal Institution and also to the Royal Society, and preliminary accounts were published in the spring of 1832. Henry saw these, probably in June of that year, and thought it best to publish at once his preliminary investigations on the same subject, so that the first published account of his work appeared in the July, 1832, number of *Silliman's Journal.* In this account he describes experiments on the production of electric currents by varying the magnetic field through a coil of wire and also an observation which he had made some years before concerning the spark which is produced when an electric circuit is broken. He noted further that these two phenomena were evidently due to the same fundamental cause. I shall describe later the details of these experiments, but I wish at this time to call attention to the probable dates at which they were performed. In view of what I have said above I do not think there can be any doubt but that his observations of induced electric currents due to a varying magnetic field were carried out in August, 1830, when he was working with his new magnet. In fact, he says explicitly that the observations were made by using this magnet. When he observed the spark produced at the breaking of a circuit and studied the effect on this spark of various forms of the circuit, it is quite impossible to say. It certainly was an early observation, and the date has been set by Joseph Henry's daughter, Miss Mary Henry, as in 1829. This is extremely probable and the claim, I think, is supported by various considerations. Miss Henry says that she often talked with her father concerning the early history of electromagnetic induction and that he always spoke as if he had discovered induced currents in 1830 and made his first observations of self-induction in 1829. I think it is very reasonable to believe that it was his observation of the electric spark on breaking a circuit and certain other phenomena to which I shall refer later, these being made in August, 1829, that led him to undertake the experiments which culminated in the discovery of induced currents in August, 1830, after he had completed his new magnet. In any case he saw so much ahead of him in August, 1830, calling for the construction of new apparatus, that he thought it best not to publish his preliminary studies, so that August, 1831, found him busy making the apparatus which he felt necessary to have available to continue his investigations. Then, again, he was stopped in his work by the opening of the school session and probably he would not have published any full account of his studies until after August, 1832, if it had not been for the fact that he saw that Faraday had already made some of the discoveries with which he was familiar as the result of his own investigations.

After Henry saw the brief notes concerning Faraday's work he took up the problem anew, repeating some of Faraday's work and extending his own, although the latter was very complete and convincing.

Having made his powerful electromagnet, which I have described above (early in the summer of 1830), he wound a coil of wire around the armature of the magnet and led the terminals of the wires to an instrument for indicating current. He noticed that when he turned on the magnetizing current there was immediately a fling of the galvanoscope needle and that when he broke the magnetizing current there was a fling in the opposite direction. He further observed that when he turned the magnetizing current off and

detached the armature there was also a fling of the galvanoscope needle, the amount of which varied according to the distance he moved the armature. Finally, he observed that when the armature was in place on the magnet and he varied the magnetizing current there was also a fling. So he felt justified in drawing the conclusion that there was an instantaneous current in one or the other direction in a helix of copper wire surrounding a piece of soft iron accompanying every change in the magnetic intensity of the iron. No statement concerning the production of induced electric currents could be any clearer than this.

His observation of certain phenomena of self-induction, probably made in 1829, was equally important. Having noticed this fact of the appearance of a spark which may have been and probably was a chance observation, owing to some break in an electric circuit, he studied the effect of having the circuit consist of a short wire or a long one and also the effect of coiling a conductor into a spiral or helix, noting the increased effect when the latter was done.

We have seen that in the summer of 1831 Henry was busy making a new magnet and new apparatus for experimental purposes. In 1832 he was elected professor of natural philosophy at the College of New Jersey, now Princeton, and he moved there with his family in November of that year. The first few years of his incumbency were occupied with the immediate duties of his chair and he did not have an opportunity to continue his studies until 1834. In November of that year Faraday published an account of his discovery of the phenomena of self-induction, and Henry's friends persuaded him that it was his duty to publish at once an account of what he had done on the same subject up to that time. This he did. He gave a verbal account of his work before the American Philosophical Society in Philadelphia at its meeting of January 16, 1835, and wrote a fuller account, which was published in *Silliman's Journal*. He had extended the observations to which I have referred above by investigating not alone the spark produced on breaking the circuit, but also the currents and the shock which accompanied the break. He had also studied the effect of introducing iron into a helix and had really made great progress. From this time on Henry worked fairly continuously and, apparently having learned his lesson in regard to publication, communicated his results as soon as he was convinced that they were definite. He had been elected a member of the American Philosophical Society in 1834 and hereafter he communicated his results to this society and they were published in its *Transactions* or *Proceedings*.

He continued for some years his study of the phenomena of self-induction and other effects of electromagnetic induction; in fact, there was a constant series of publications on the subject till his election as Secretary of the Smithsonian Institution in December, 1846.

In one series of experiments he investigated the effect of a discharge of a Leyden jar through his primary coil in producing induced currents in neighboring conductors and was ultimately led to the proof that such a discharge was oscillatory. (Conviction that this was the character of the discharge had been expressed previously in 1827, by Savaray, but this fact was not known by Henry.) He was able to prove also that this inductive action produced by discharges through a primary conductor was felt at considerable distances, certainly as far as two hundred feet. This is the first experiment on record of electromagnetic waves (in 1842).

One of his most important studies dealt with the effect of introducing sheets of conductors between his primary and secondary coils (in 1838). One of the main interests in this study lies in the fact that Henry's observations were quite contrary to those announced by Faraday. Henry showed that the introduction of a plate of copper cut off completely the inductive action, whereas Faraday had found that there was no shielding effect. The explanation of the discrepancy, which Henry, himself, was able to make, lies in the fact that Henry's observations were on what we nowadays call electromotive force, whereas Faraday in his investigations was concerned with the quantity of induced current. This fact emphasizes the difference in the experimental equipment of the two investigators. Henry had practically no measuring apparatus and had to content himself with such observations as sparks and the shocks received when discharges were passed through his body; Faraday, on the other hand, had a well-equipped laboratory. It is extremely interesting as one reads the papers by the two great physicists to see how they express themselves without having the benefit of the knowledge which came when Ohm's law was known. I think of the two men Henry had perhaps a clearer understanding of the essential features of induced currents, but I could not give conclusive proof of this. Both men were aware of the fact that there was one quality of the current which depended upon the rate at which the magnetic field is changed, this being what determines the shock in the muscles and the distance at which a spark will occur in a broken circuit, and that there was another property of the current depending upon the total change in the field, which determined the fling of the galvanometer needle. The former was independent of the material of the conductor, while the latter varied with it. It was not, however, until the work of Lenz and of Neumann that all the diffi-

culties were removed and all the phenomena expressed in one simple equation.

One interesting experiment of Henry's in this connection was to show that although there were differences in the shock produced in the secondary coil when the current was made or broken in the primary, there was no difference at all in the fling of the galvanometer needle. This observation led him to a very careful study of the phenomena associated with making and breaking a circuit.

The discrepancies between the observations of Henry and of Faraday, depending upon the fact that the former was observing as a rule electromotive force, while the latter was measuring the quantity of current, reminds one very much of the discrepancies which existed in the early history of mechanics, discrepancies which were only cleared up by the mathematical work of D'Alembert. Mechanics had its origin, as is known to you all, in the work of Galileo, Newton and Huyghens, and in the century that followed their first publications a controversy arose as to the proper measure of those agencies in nature which produce the changes in velocity of a body. One school of writers insisted that the proper measure of the effect of such agencies was to be found in the difference in the squares of the velocities of the body at the beginning and the end of the action. Another school insisted vehemently that the effect should be measured by the difference in the velocity. If this dispute were stated in modern language it would be somewhat as follows: Is the effect to be measured by the change in the kinetic energy or by the change in the momentum? It was D'Alembert who first showed that neither one of them was the proper measure, because the change in the kinetic energy is equal to the product of the force by the distance through which the body moves under the action of the force, while the change in the momentum is equal to the product of the force by the time during which the body is under the action of the force. Or, it may be said, if one wishes to, that both schools were right and that they were looking at the two sides of the shield as in the ancient fable. Something of the same kind may be said in regard to the work of Henry and Faraday. Their research work was absolutely trustworthy, but their interpretation of this could not be completely satisfactory until the work of Ohm was appreciated and until the mathematicians had completed their study.

I can add but little to what is well known concerning Henry's qualities as an investigator and administrator. As one reads the various papers contributed to the memorial volume devoted to his life, one is struck by the universal admiration for his broad philosophy, his accuracy of observation, his brilliant intuitions and his devotion to the cause of science in its widest interpretation. He was unselfish to a marked degree. He was not interested the faintest in personal advancement or in advancing claims for discoveries or inventions. His sole purposes in life were to interpret nature and to diffuse knowledge among men. Beyond any doubt he is the outstanding figure in the history of the scientific life of America.

THE RELATIONSHIPS OF THE NATURAL SCIENCES AND THE SOCIAL SCIENCES IN AGRICULTURAL EDUCATION IN THE UNITED STATES[1]

By Dean W. C. COFFEY
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IN this country the program of agricultural education, as it relates to both teaching and research, centers in the natural and social sciences. It is perhaps safer and wiser to describe the functions of these sciences in this program in rather broad and general terms, as no two persons would likely fully agree on any exhaustive statement about either of them.

Natural science, in its application to agricultural and pastural products intended for consumption, has to do with improving and increasing production by reducing the amount of time or effort necessary to produce a unit of product and by making more resources for production available. It is addressed to the soil for such purposes as increasing balancing and maintaining its plant food elements, controlling its moisture content and improving its condition with respect to tillage. It is applied to plant life as it relates to agriculture and deals with the breeding and selection of plants and their adaptation to given conditions and needs. It has to do with their culture and with means of protecting them from the ravages of disease and insect pests. Along practically the same lines it deals with animal life in agriculture. And it also has to do with the interrelationships of soil and plants and animals.

Social science deals with human wants and primarily those which are satisfied only by associated or

[1] Address of the retiring vice-president and chairman of Section O—Agriculture, American Association for the Advancement of Science, New Orleans, December 29, 1931.

group action. Hence we often think of it as a science that deals with society as a whole. In agriculture we restrict its application, in large part, to that portion of society known as the rural population. The social science subjects most emphasized in agricultural education are economies and sociology, but it should be borne in mind that such subjects as history, government and politics belong in this field. It is well, too, not to overlook the fact that a considerable amount of the subject-matter in agricultural economies is directed to individual rather than to group wants. Reference is here made to teaching and research aimed at such matters as adjustments on the individual farm for the purpose of increasing individual efficiency and advantage. While there is a sharp division between individual and group wants at some points, they merge at others; therefore we can not confine the application of social science solely to group wants, but when the objective is an individual rather than a group want, the results sought are very similar to, if not identical to, the results desired when natural science is the agency used.

Social science is concerned with problems of distribution of wealth and income. It has to deal with questions of credit finance and taxation. Price is an organizer of economic activity and a factor in distribution and it becomes a function for a subject like agricultural economies to consider price factors and movements as they relate to agricultural products.

It is a function of a subject like rural sociology, through a technique all its own, to give attention to the improvement of human factors in agriculture to the end that there shall be a sound development of rural social policy. Attention must be given to these human factors if the improvement of agriculture itself is not to be defeated. "The capacity of the rank and file of farmers to receive instruction can be stimulated and greatly increased through processes of socialization of families, groups and communities so as to effect a nicer adjustment of individuals to a larger and more complex organization of life."[2] Dean C. B. Hutchison, of the College of Agriculture, University of California, recently gave in broad and general terms the following as the function of social science research as it applies to agriculture.[3] "It relates to the balancing of production and consumption, the ways and means of making adjustments and changes, and the distribution of the benefits from scientific progress among the members of society."

Agricultural institutions for teaching and research are more at home with subjects in natural science than with those in social science, the reason being that the national system of agricultural education was developed by placing emphasis primarily on the natural sciences. There was a logical basis for this method of development. The need for harnessing and overcoming the forces of nature was much more obvious to the farmer and even to the educator than were needs along social lines. The farmer was often squarely confronted by the limitations of cold, drouth, disease, pests, low producing powers of plants and animals, etc. As to these needs he had no trouble in expressing himself, whereas he was more or less inarticulate regarding his social needs because he could not clearly comprehend them. Moreover, a number of the present rural social problems did not develop in acute form until the system of agricultural teaching and research had become well established.

When the agricultural colleges were provided for by the federal government through the Morrill Act of 1862, a large percentage of American farmers were operating on a self-sufficient basis in considerable degree and continued so to do until well up toward the close of the past century. During most of this period rural standards of living were, in large part, determined by the capacity to be self-sufficient. The farmer's attitude was extremely individualistic. The family then enjoying a high standard of living was the large family that produced crops successfully, converted a portion of them efficiently into family needs, sold enough to pay for comparatively light overhead costs, saved a little money for investment in more land and managed to satisfy social and educational wants within the home and the closely circumscribed community.

We had of course developed the production of staple crops far in excess of the consumption needs of the agricultural population before the close of the past century. We had passed through periods of discouragingly low prices, but even so production remained the paramount idea in agriculture. With our own population growing so rapidly, we looked forward to great increases in population at home and abroad. We were impressed by the theory of Malthus concerning population, and hence felt that the only check to increases in population would result from restrictions in food, clothing and shelter. We visioned the hungry maws of England, Germany and other European countries clamoring for more food and as we turned the century the public mind entertained a fear of being unable to produce enough to feed the oncoming millions. There were those amongst the intelligentsia in agricultural education who saw the time only shortly ahead here in expansive America when we would have to limit our consumption of meat materially in order to have enough

[2] From monograph, "Rural Sociological Research in the United States," 1927.

[3] From paper read before the section of experiment stations, annual meeting of Association of Land Grant Colleges and Universities, Chicago, Illinois, November, 1931.

food from our available acres to feed our own people.

In sum, our whole agricultural philosophy was faced toward production and we were so occupied with the problems surrounding production that we pretty largely allowed social and economic problems to take care of themselves. Some of us may have thought that if we could adequately discover ways and means of overcoming controllable handicaps to production, we would solve our rural social and economic problems. For the natural science worker in agriculture almost invariably had social and economic values in mind as he has applied himself to his tasks. It was not a matter of entirely overlooking these values, but rather a matter of opinion as to how they might be realized.

While extensive consideration of social and economic problems is a comparatively recent development in agricultural education it would be hardly true to say that the problems themselves are of recent development. But much has transpired since the beginning of the present century to bring them to the fore with greatly added emphasis, and doubtless some new problems have developed. They now stand out so prominently that it would be quite impossible to deny them large consideration in any intelligent program of education.

Many things have happened to bring the economic and social problems of agriculture to the fore, but of them all two seem to be more significant than any of the others. One is the stupendous augmentation of machine economy which has occurred during the past two decades; the other is the spread of birth control. The first has greatly increased the producing power of the individual farmer and has extended crop areas by utilizing types of land which probably could not have been cropped economically without power machinery operating on an extensive scale. Then, too, the acceleration of the machine economy throughout all groups of society has had a most noticeable effect on the habits of the people, and by encouraging a more sedentary type of living has reduced the per capita consumption of food and has had an effect on the types of food preferred.

Although the machine age has been on the way for many years, what has been referred to as the new machine age ushered in at about the time the world war opened was not foreseen in its gigantic proportions and far-reaching consequences. Apparently none of us could foresee the extent to which human hands and backs would be replaced by labor-saving machinery. None of us could foresee that within a decade 30,000,000 acres of agricultural land would have to be devoted to some new use because of the decimation of the horse population. Nor could we foresee that while this would be happening the total amount of land under cultivation would be materially increased, all of which would be brought about by the greater adaptation of mechanic power and machines to farm practices.

It was no easier to foresee the movement toward birth control than the sudden upsurge of the machine economy. Either as a national or world-wide movement it was entirely outside of our thinking. In the November, 1931, *Forum*, Louis I. Dublin says: "In years preceding 1880, the birth rate in most countries was 35 to 40 per each 1,000 population. This figure is not very far from the upper limit of human reproductive capacity in a normally organized population. To-day with a very few important exceptions, as in Russia and Italy, the rates vary from 15 to 20 per thousand." Dublin goes on to say that evidence points to the conscious control of propagation through contraceptive methods as the new element which has altered the situation with respect to birth rate. He further says, "The outlook for the future is a wider application of these methods and toward their dissemination to all peoples. Whether we like it or not we look forward to much lower birth rates in all parts of the world." It therefore seems necessary to reverse the way we thought about population during the boom days of agricultural education in the first decade of the century. Again, Dublin says: "We of the United States are evidently not destined to become the great reservoir of humanity where liberty and prosperity are to reside side by side. Instead, we shall, from now on, accustom ourselves to hear questions like these. How long will America continue to increase in numbers; what will its maximum population be; how rapidly will the population decline once it begins to fall; what effect will this decline have on age distribution and therefore upon business and employment conditions; how will it affect America's position of leadership abroad? In short, now that one frontier has completely disappeared a new era of the history of the nation has set in."

We are all aware, of course, of the fundamental cause underlying birth control, namely, that with our changing modes of life there are other things more desirable than the expensive luxury of a large family. Even on the farm children are no longer an economic asset, but rather an economic liability of considerable magnitude. In consideration of the way in which we are headed there is small probability of their becoming an economic asset. Therefore, Dublin is probably correct in his views about declining birth rates.

What has happened and is happening with respect to machine economy and birth control clearly shows that we can no longer count on increase of population to take care of increases in agricultural production which may be brought about by the agencies within the field of natural science. The massive

forces of the machine economy alone are too powerful for any possible rate of population increase to cope with our power to expand production, to say nothing of the fact that the rate is declining. It is clear that, from now on, the matter of balance between the production and the consumption of agricultural products will be of vital importance. In many respects it will be a delicate balance and any onrushing movement to increase production, as has been the case in the greater application of machine power, will cause serious maladjustments. From now on rural people must keep their eyes upon both the production and the disposal of their products. Already some of them have called attention to their conviction that our agricultural institutions are emphasizing production to the neglect of distribution. They have gone so far as to assert that we might very well take a holiday on matters pertaining to production in order that we may catch up with our information on distribution. In this they are wrong because much of the work in natural science is not directed toward larger production and they are unaware of the complaint that would be made were we to follow their suggestion. But their criticism carries significance, nevertheless, for it indicates their realization of how impossible it is for us to continue with an unbridled policy of production. Consequently, the long-established natural science forces in agricultural education should seek and welcome the development of social science as it may be applied to agriculture.

If we are to secure the full benefits of natural science teaching and research, under present conditions in agriculture, we must have a sound and adequate development of social science. It is the science upon which we must rely for finding the ways and means of making adjustments, of balancing production to the needs and desires of the people. Unless these things are done, natural science teaching and research will be greatly weakened as potent influences in rural life. If they are done, if production is expanded only as fast as there is need for it, then the gains or benefits from natural science can be turned into better living rather than in surplus production.

We should not conclude that study and research in social science alone will not improve conditions. Any such contention is futile. The sources of wealth are to be found in goods and services. The farmer's source of wealth lies in goods which he produces, hence he must maintain an active interest in production. In its productive phases agriculture is a biological industry and hence as subject to change and variation as the biological forces themselves. The natural science worker in agriculture need have no fear of ever being out of a job, for his services will always be in demand. He will continue to be called upon to make discoveries and to work out their applications. It will remain for social science to lead in determining where, how and how rapidly they shall be applied. So for best results, in fact for safe results, it becomes apparent that a knowledge of natural science must dovetail with a knowledge of social science. In turn each becomes the handmaiden of the other.

We would agree, I think, that the more intelligent American farmers have come to appreciate the value of natural science research as it applies to their individual businesses. In order that social science may pull itself up to the level of the accomplishments of natural science, it must engage in rather large-scale social and economic experiments which involve individuals who are acquainted with what is being attempted and who are willing to subject themselves to experimentation. Reference is made to some of the large experiments in cooperative marketing and to new ventures in land policy such as the state of New York is undertaking by acquiring abandoned farm land adjudged to be unfit for agriculture. One object in the acquisition of these lands is to prevent the waste of human effort and capital on land wholly unsuited to farming. Another is to effect savings in expenditures for schools, roads and local government. The time has come when we can scarcely avoid experiments of this sort, although it is doubtful if we should attempt to be as ambitious as Italy or Russia along these lines. The confidence that has been built up in research through natural science will help to pave the way for confidence in researches of this sort. This does not indicate a relationship between the sciences but rather a service one has rendered the other.

Attention has been called to the fact that natural science workers in agriculture have almost invariably had social and economic values in mind. There is, of course, a social reason for encouraging greater production. But aside from this, the worker in horticulture who deals with fruit and vegetable growing or the growing of ornamental plants may have little notion of boosting the total production in agriculture, but rather the hope that he may make some contribution to the satisfaction of living. The agricultural engineer, who clears the way for farm electrification, may talk all the while about how electricity on the farm can make for the efficiency of production, while deep down in his heart he may be hoping to make farm life more cheerful, happy and satisfying. We must bear in mind, too, that unless we run to seed on the subject, there is a social satisfaction in attaining efficiency in production. Many a man has spent his life in producing superior purebred live stock, fully realizing that he might have made a little more money by producing for the open market.

Many a man has lost a little money by adding increments of labor to his enterprise purely for the sake of doing the job well. Many a man has been carried on through hours of discouragement in agriculture by his knowledge of his ability to perform creditably in the processes of production. So, after all, there are no hard-and-fast lines between the two fields of science. To attempt to draw such lines would hamper both natural and social science effort.

It has frequently been difficult for natural science workers to see an important place for social science in the scheme of agricultural teaching and research.[4] In other words, they have not had a genuine respect for it as a science. Since social science is of later development than natural science in agricultural education, particularly, there was a lag in the refinement of its technique. Then, too, the complex interrelationships of social and economic phenomena are hard to understand and measure. Only in recent years have we come to feel that scientific research can be applied to these phenomena. Descriptive studies have been under way for a long time, and "qualitative analyses of cause and effect relationships based on assumptions, mainly because data were not available, have been made for more than one hundred years." Through their determination to discover principles and to secure reliable measurements of relationships the workers in social science are winning the respect of the natural scientists. Nothing else could do quite so much toward bringing about the kind of cooperation needed between the workers of the two fields.

Healthy cooperation between natural and social science can be fostered by placing adequate emphasis on the social sciences in the agricultural curriculum. Students should not be graduated without some basic knowledge of social and economic principles any more than they should be graduated without basic knowledge of the fundamental principles of natural science and particularly the biological sciences. This can be accomplished without lessening the emphasis given to natural science—in fact it is being done to a limited extent by giving less time to courses that are nothing more than "glorified practicums." Such courses are not without value, but their prominence must wane in any sound program of agricultural education. It has long since been demonstrated that the acquirement of a number of practical skills in college does not produce a finished farmer, much less a capable leader in agricultural thought.

Nor have we met the situation when we train technical farmers and technical research workers who perhaps receive no more than one or two elementary courses in economics which so often give the impression that society is governed by immutable laws and, therefore, nothing can be done about it. More training in social science than this is needed and it should be the kind of training which demonstrates that the laws governing society are no more rigid than the laws governing the forces of nature, and that an understanding of these laws can contribute to social progress quite as much as an understanding of natural laws can contribute to technical progress.

The natural science worker has implicit faith in his task. In this he is fully justified, for he has to his credit a great record of achievement. He has worked untiringly and in most cases in the spirit of altruism, or at least not in the spirit of selfishness. The world has acclaimed him and has increasingly accepted his leadership. We increasingly speak of the gains the scientific temper has brought us. But in spite of all the advantages we can mention, we must admit that, for some reason, the world is sick, that agriculture is depressed, that we have overreached ourselves in production, that by gaining control of the forces of production, we have apparently lost economic control and brought social distress to ourselves. Some would sarcastically and wrongly say that through technical efficiency we have bankrupted ourselves and broken our social morale. Evidently we are confused, muddled, lost even in our thinking. The way out is not to call a retreat in technical efficiency. The way out is to realize that the results of the discoveries of natural science must be intelligently handled by society, that it is not natural science which is causing grief, but rather the unintelligent application of its discoveries by society.

With the automatic device for handling increase in agricultural production going into the discard, namely, increase in population, it becomes clear that our production can run wild, and probably will, unless a science is developed within our rural society for the purpose of turning efficient production into channels of better and happier living for every one.

On the one hand, our agriculture needs a science to make us intelligent and efficient in our processes of production; on the other hand, it needs a science to make us intelligent and efficient in distributing our production and adjusting it to the logical demand for it. If the amount of production greatly exceeds the demand for it, interest in intelligent and efficient production and the science underlying it is sure to sag. If production should be unintelligent and inefficient, no system of distributing and adjusting production to demand could bring prosperity and better living to rural people. In these very simple terms, the essential relationship between the natural and social sciences in agriculture may be expressed.

[4] The discussion in this and the two succeeding paragraphs closely parallels discussion to be found in the paper of Dean C. B. Hutchison read before the section of experiment stations, annual meeting of Association of Land Grant Colleges and Universities, Chicago, Illinois, November, 1931.

SCIENTIFIC EVENTS

THE IMPORTATION OF SCIENTIFIC SPECIMENS AND APPARATUS INTO GREAT BRITAIN

As the result of a report by the Association of British Zoologists, the council of the British Association in February, 1931, appointed a committee to consider action with a view to the amelioration of the customs regulations affecting the importation of scientific specimens and apparatus. Following upon discussion between officers of the association and the Customs House authorities, the latter have most kindly supplied the association with a memorandum on the reliefs from customs duties on scientific instruments and cinematograph films, and from the import prohibitions on plumage likely to be of use to scientific workers, together with a note on procedure in respect of the importation of scientific specimens preserved in spirit.

The memorandum on scientific instruments and cinematograph films was supplied confidentially to enable the association to advise *bona fide* scientific workers, but not for general publication, since some of the relaxations are extra-statutory and liable to modification or withdrawal as the interests of the revenue may demand. The British Association is, however, prepared to advise on specific applications from scientific workers or societies. Enquiries should be addressed to the secretary of the British Association at Burlington House, London, W.1.

It emerged in the course of discussion that some of the difficulties which have been encountered by scientific workers under the customs regulations might have been avoided by previous communication with the Board of Customs and Excise.

THE UNIVERSITY OF PORTO RICO AND THE U. S. DEPARTMENT OF AGRICULTURE

TENTATIVE arrangements for cooperation in research and extension work between the U. S. Department of Agriculture and the College of Agriculture of the University of Porto Rico, at Mayaguez, were made by Dr. C. W. Warburton, director of extension work, and by Dr. James T. Jardine, chief of the Office of Experiment Stations, in the course of a brief visit to the island, from which they returned on December 21. This cooperation was authorized by an act of congress, approved on March 4, which provided that the several experiment station and extension acts should apply to Porto Rico, beginning on July 1, with an initial appropriation of $15,000 for research and $50,000 for extension work, these sums to be increased respectively by $5,000 and $10,000 annually until the totals to which Porto Rico is entitled under the federal act are reached.

The insular government of Porto Rico now maintains an agricultural experiment station and an agricultural extension service, and these will be included in the cooperative agreement. For many years this department has maintained an experiment station at Mayaguez, the work of which will now be coordinated with that of the cooperative insular station. The extension work on the island will be expanded to include home economics and boys' and girls' club work.

"Though Porto Rico is feeling the effects of the world-wide depression severely, harvesting of the largest crop of sugar ever produced on the island is now beginning, and the unemployment situation there will be greatly relieved," according to Dr. Warburton. "Failure to sell the 1931 tobacco crop at satisfactory prices has curtailed planting for the 1932 crop, vigorous efforts having been made in some sections to secure an agreement among growers to plant no tobacco at this time. The 1931 production of coffee was very low, this crop not yet having recovered from the 1928 hurricane. Agricultural and educational agencies are conducting a vigorous campaign for the production of larger supplies of food crops for local use, in an effort to feed the rapidly increasing population and reduce the imports of foodstuffs."

TO GUADALUPE ISLAND

THE fascination of studies in island life is answerable for the latest expedition to Guadalupe, the little-visited volcanic island 140 miles west of northern Lower California. Mr. Templeton Crocker, of San Francisco, recently visited this remarkable island on his yacht *Zaca* in the interest of the California Academy of Sciences and spent three days (November 14 to 16) exploring the island and adjacent waters. Mr. Crocker was accompanied by Mr. John Thomas Howell, botanist of the California Academy of Sciences, who conducted the scientific studies. From several stations on the island collections were obtained of insects, birds, fishes, plants and land and marine shells. Although the season was at low ebb for extensive plant collections, the wet season just having begun, thirty-three collections of native vascular plants were made, four of which have not been known heretofore from the island. In addition to these vascular plants many mosses, hepatics, and lichens were collected. Among the specimens of special interest brought back is a 300-pound Giant Black Marlin Spearfish (*Makaira marlina*), an interesting big game fish recently described from the Pacific off Cape San Lucas. This specimen was taken by Mr. Crocker personally and brought to the academy, where it is now being prepared as a permanent exhibit to be placed in the Steinhart Aquarium.

A census was taken of the Guadalupe elephant seal by Mr. Crocker and Captain D. Rotch. In the large herd on the west side of the island near Elephant Rock approximately 900 individuals were counted, of which about one third were males. On a small beach on the east side of the island, near the Northeast Anchorage, 27 seals were numbered. At neither of these beaches was any seen dead, and the general condition of the animals seemed to be good. The valuable data and collections made on this expedition to Guadalupe Island make it very gratifying to know that a business man of Mr. Crocker's prominence has shown such a keen interest in the pursuit of scientific field studies.

JOHN THOMAS HOWELL
CALIFORNIA ACADEMY OF SCIENCES

PROFESSORSHIPS AT YALE UNIVERSITY IN MEMORY OF GIBBS AND SUMNER

THE memory of two distinguished professors of Yale University, Josiah Willard Gibbs and William Graham Sumner, has been perpetuated by the establishment at the university of the Josiah Willard Gibbs professorship of mathematics and the William Graham Sumner professorship of the science of society, both made possible by funds pledged in 1927 as a part of the Yale Endowment Fund and now paid to the university for this purpose.

Two members of the Yale faculty have been appointed to these professorships. The first Gibbs professor of mathematics is Dr. Ernest William Brown, previously Sterling professor of mathematics; the first Sumner professor of the science of society is Dr. Albert Galloway Keller, now professor of this subject.

Professor Brown, who has been a professor at Yale since 1907, is an authority on celestial and especially lunar mechanics. A graduate of Cambridge University (Christ's College), he has won the Royal Medal of the Royal Society, of which he is a fellow; the gold medal of the Royal Astronomical Society, and the Bruce Medal of the Astronomical Society in this country. He is a member of the National Academy of Sciences, a fellow of the American Academy of Arts and Sciences, and is a former president of the American Mathematical Society. Professor Keller has been a member of the Yale faculty since 1899. He is the author of several authoritative works in the field of social science. Professor Sumner had spent his later years collecting material for a monumental work to be known as "The Science of Society." On his death, his successor at Yale, Professor Keller, took up the work, and after twenty-five years of preparation, published four volumes under this title in 1927 through the Yale University Press which had previously issued four volumes of Sumner's collected essays under the editorship of Professor Keller.

The announcement from the office of the secretary of Yale University characterizes the work of Gibbs as follows:

Professor Josiah Willard Gibbs, who has been called "the first physical chemist," graduated from Yale College in 1858. He received the doctorate from Yale in 1863, and after tutoring at the university for one term, spent three years in Europe. In 1871 he was appointed professor of mathematical physics at Yale, a position which he held until his death in 1903. Many honors and degrees and medals came to him, among these being the Rumford Medal from the American Academy and the Copley Medal from the Royal Society of London. Although he was not in the strictest sense of the word a chemist, Professor Gibbs is generally considered to have made the greatest single contribution to theoretical chemistry made by an American. In 1876–78 he published a series of papers on "The Equilibrium of Heterogeneous Substances." Because of the strictly mathematical treatment of the subject and the prevailing lack of familiarity of chemists with higher mathematics, its value was not recognized, nor was the great principle involved used by chemists for several years.

The great value of the "Phase Rule," a principle evolved from Professor Gibbs' mathematical discussion, is now acknowledged by all working in the field of theoretical chemistry. The layman, however, does not realize the great practical use which is made of this principle first developed by Gibbs. The familiar carbon-iron diagram, which is the basis of the metallurgy of steel, is based upon it. The whole subject of alloys and the science of metallography is founded on it. The work which has explained the constitution of portland cement was only possible through this rule. These are but a few examples of its value and versatility.

AWARD OF THE FIRST RICHARDS GOLD MEDAL TO PROFESSOR ARTHUR A. NOYES

THE Richards Gold Medal for conspicuous achievement in chemistry has been awarded by the Northeastern Section of the American Chemical Society to Professor Arthur A. Noyes, director of the Gates Chemical Laboratory of the California Institute of Technology, Pasadena, according to an announcement made by Professor William P. Ryan, chairman of the section. The medal will be presented at the time of the annual meeting early next May.

The Richards Medal was established by the Northeastern Section in 1929 to commemorate the many fundamental contributions made to chemistry by the late Theodore William Richards, who was professor of chemistry in Harvard University for over twenty-five years and the only American chemist to receive the Nobel prize. He was a member of the Northeastern Section for thirty years. A trust fund of $10,000 to endow the medal has been raised by a committee consisting of Drs. Lyman C. Newell, Ar-

thur D. Little and James F. Norris. The medal was designed by Mr. Cyrus E. Dallin, who was an intimate friend of Professor Richards.

Dr. Arthur Amos Noyes, the first recipient of the Richards Medal, was born at Newburyport, Massachusetts, in 1866. After graduation from the Massachusetts Institute of Technology he studied in Leipzig, where he received the degree of Ph.D. in 1890. He taught at the institute from 1890 to 1919, and was acting president 1907–1909. He went to the California Institute of Technology in 1915 to assist in its organization and took up permanent residence there as professor of chemistry in 1919. In recognition of his contributions to chemistry he has been elected to numerous scientific societies, has received many honorary degrees and has been awarded several medals including the Davy medal of the Royal Society of London. He is the author of books in qualitative analysis, organic chemistry and physical chemistry, and has published a large number of articles on his original researches in theoretical, analytical and organic chemistry in American and foreign journals. Professor Noyes was a charter member of the Northeastern Section of the American Chemical Society.

SCIENTIFIC NOTES AND NEWS

IN the British New Year honor list science is recognized in knighthoods conferred on Mr. George Buckston Browne, the donor of Darwin's house at Down to the British Association and of generous gifts to medical research; Dr. H. H. Dale, secretary of the Royal Society and director-in-chief of the National Institute for Medical Research; Dr. Patrick Geddes, late professor of botany at St. Andrews and professor of sociology and civics at Bombay, and Mr. H. S. Wellcome, the founder of the Wellcome Research Institution and the Wellcome Tropical Research Laboratories at Khartum.

PROFESSOR CHARLES RICHET, the physiologist, has been elected vice-president of the Paris Academy of Sciences for 1932 and will become president in 1933.

DR. HUGO ECKENER, commander of the dirigible *Graf Zeppelin*, received on January 16 the award of the 1931 medal of the International Aeronautic Federation.

M. LUCIEN CUÉNOT, professor of zoology at the University of Nancy, who in 1918 was elected a correspondent of the Paris Academy of Sciences, has now been elected a non-resident member to succeed the late M. Cosserat.

PROFESSOR E. L. NICHOLS, of the department of physics of Cornell University, has been elected an honorary member of the Association des Ingenieurs de l'Eclairage, Paris.

AT a special meeting held at the St. Louis Medical Society, by friends of Dr. Amand N. Ravold in recognition of his completion of fifty years in the practice of medicine, a plaque was presented to him by Mr. Victor Holm. Dr. Augustus G. Pohlman, Washington University School of Medicine, extended greetings; Professor Arthur I. Kendall, of the department of research bacteriology, Northwestern University Medical School, Chicago, gave an address on "Fifty Golden Years"; Dr. William W. Graves, "Amand Ravold the Builder," and Dr. Joseph Grindon, "Amand Ravold the Man."

MR. WILLIAM S. LEE, of Charlotte, North Carolina, known for his work on high-tension hydro-electric power development and transmission, has been elected president of the American Engineering Council. He is president of the W. S. Lee Engineering Corporation and vice-chairman of the Duke Endowment.

DR. SCOTT TURNER, chief of the U. S. Bureau of Mines, has been nominated for the presidency of the American Institute of Mining and Metallurgical Engineers for 1932.

OFFICERS elected at the twenty-fourth annual meeting at Atlantic City of the American Institute of Chemical Engineers were as follows: J. V. N. Dorr, *president;* A. E. Marshall, *vice-president;* F. J. LeMaistre, *secretary;* Martin H. Ittner, *treasurer;* David Wesson, *auditor,* and H. C. Parmelee, Willard Dow, J. G. Vail and R. T. Haslam, *directors.*

DR. R. S. BASSLER, since 1929 head curator at the U. S. National Museum and professor of geology at George Washington University, was elected president of the Paleontological Society of America and vice-president of the Geological Society of America at the recent meetings at Tulsa, Oklahoma.

AT the annual meeting of the Mineralogical Society of America, held at Tulsa, from December 29 to 31, the following officers and fellows were elected: *President,* Alexander N. Winchell, University of Wisconsin; *Vice-president,* Joseph L. Gillson, E. I. du Pont de Nemours and Company, Delaware; *Treasurer,* Waldemar T. Schaller, U. S. Geological Survey; *Secretary,* Frank R. Van Horn, Case School of Applied Science; *Editor,* Walter F. Hunt, University of Michigan; *Councilor,* 1932–1935, William J. McCaughey, the Ohio State University. The following fellows were elected: Dr. Olaf Anderson, Research Laboratory, United States Steel Corporation, Kearny,

New Jersey; Dr. Richard C. Emmons, assistant professor of geology, University of Wisconsin, and Dr. James E. Hawley, professor of mineralogy, Queen's University, Canada.

AT the annual meeting of the American Society of Tropical Medicine held in conjunction with the meeting of the Southern Medical Association at New Orleans, from November 18 to 20, the following officers were elected for the ensuing year: *President,* Dr. Frank Smithies, Chicago; *First Vice-president,* Dr. E. L. Walker, San Francisco; *Second Vice-president,* Dr. G. R. Callender, Washington; *Secretary-treasurer,* Dr. Henry E. Meleney, Nashville; *Assistant Secretary,* Dr. Dámaso de Rivas, Philadelphia; *Councillors,* Dr. Dámaso de Rivas, Philadelphia; Dr. B. C. Crowell, Chicago; Dr. E. B. Vedder, Washington; Dr. E. C. Faust, New Orleans, and Dr. H. C. Clark, Panama.

AT the meeting of the Botanical Society of America in New Orleans the Mycological Section voted the establishment of an independent mycological society. Dr. William H. Weston, of Harvard University, was elected president and Dr. H. M. Fitzpatrick, of Cornell University, secretary. A committee of five, including the officers mentioned, was appointed to draw up a constitution and by-laws and negotiate with the New York Botanical Garden looking toward an agreement whereby *Mycologia,* now published by the New York Botanical Garden, can be adopted as the official organ of the newly formed Mycological Society of America. The results of these negotiations will be reported at the meeting held in connection with those of the American Association for the Advancement of Science in Atlantic City next winter.

THE dismissal of Dr. Carl C. Taylor, dean of the Graduate School at the State College of Agriculture and Engineering of North Carolina on June 8, 1931, is found by the committee on academic freedom and tenure of the American Association of University Professors to be "unusual" and to have been caused by other motives than the officially announced necessity of reduced budget. The summary action of the trustees of the college in discharging "the most distinguished member of the faculty" is analyzed by Professor Holland Thompson, of the College of the City of New York, a native of North Carolina, who made the investigation at the request of the committee, and is described by him as arising from several possible causes. The determining factor, however, is declared to be the personal "tension which had developed" between President E. C. Brooks and Dr. Taylor over several years.

DR. ELLIOT CARR CUTLER, director in surgery at the Cleveland Lakeside Hospital and professor of surgery in the Medical School of Western Reserve University, has been appointed Moseley professor of surgery at the Harvard Medical School and surgeon-in-chief at the Peter Bent Brigham Hospital, to succeed Dr. Harvey Cushing, whose resignation takes effect on September 1 after a service of twenty years.

DR. GEORGE BURGESS MAGRATH, medical examiner for Suffolk County, has been appointed the first holder of a new chair of legal medicine which has been founded at the Harvard Medical School. The foundation is the gift of Mrs. Frances Glessner Lee, of Littleton, New Hampshire, and has been established to support a resident professor of legal medicine and to provide for one or more lectures by visitors each year. A provision of the gift provides a plan to accumulate at the medical school a file of cases on the subject, and it is hoped that part of Dr. Magrath's records of nearly 19,000 cases may form the nucleus of this collection.

DR. JOHN CALVERT DONALDSON, associate professor of anatomy in the School of Medicine of the University of Pittsburgh, was promoted to a professorship in anatomy at the beginning of the current term.

MR. O. L. V. DE WESSELOW has been appointed to the chair of medicine at the University of London, tenable at St. Thomas's Hospital Medical School. Since 1920 he has been chemical pathologist to St. Thomas's Hospital.

IN reporting in a recent issue of SCIENCE that Dr. J. S. Plaskett has been awarded the Catherine Wolfe Bruce Gold Medal for the year 1932 it was stated in error that he had been appointed recently Savilian professor of astronomy at the University of Oxford. It is Dr. Plaskett's son, Dr. Harry H. Plaskett, who has become professor at Oxford.

MR. GEORGE T. SANFORD, assistant director of reclamation economics, has been appointed chief of the engineering division of the Bureau of Reclamation.

THE HONORABLE CHARLES G. DAWES, who will resign in spring as ambassador at London; Robert Walton Moore, member from Virginia of the House of Representatives from 1919 to 1931, and Senator William Cabell Bruce, Maryland, member of the upper house from 1928 to 1929, have been nominated by the Senate as members of the Board of Regents of the Smithsonian Institution.

DR. EDWARD R. WEIDLEIN and Mr. William A. Hamor, of the Mellon Institute of Industrial Research, Pittsburgh, have been appointed by Governor Pinchot to membership on the Greater Pennsylvania Council, which will be concerned with the development of agriculture, the industries, recreational facilities and com-

munity welfare in the state. Mr. Hamor has been made a member of the council's executive committee.

PROFESSOR J. O. SNYDER, of the Stanford zoological department, was recently appointed to the office of state fish culturist by the California Department of Natural Resources. He succeeds Mr. W. H. Shelby, a member of the Fish and Game Commission for the past forty-five years.

DR. E. B. COPELAND, associate curator of the herbarium and lecturer in botany at the University of California, has been granted leave of absence from March 1 to June 30, the leave to be extended later to January 31, 1933.

MR. ALVIN SEALE, superintendent of the Steinhart Aquarium of the California Academy of Sciences, has been placed in charge of the scientific work for an expedition to secure aquatic specimens in the waters around Central and South America.

AT the recent annual meeting of the British Science Masters' Association at the Imperial College of Science, London, Mr. Jocelyn Field Thorpe, F.R.S., professor of organic chemistry in the Imperial College of Science, gave an address on the new vista which organic chemistry is opening up, and its bearing on good health and long life.

MR. A. F. MOORE, having examined four high peaks in South West Africa with reference to their availability for solar-constant stations, reports to the Smithsonian Institution that none of them are as favorable as Montezuma, Chile, though one or two may be somewhat superior to Mount Brukkaros, South West Africa. The observatory at Mount Brukkaros has been discontinued and its equipment shipped to Washington. Messrs. Moore and Sordahl will now examine conditions at Mount St. Catherine in the Sinai Peninsula, ten miles south of Mount Sinai.

DR. JOHN HAMMOND, of the department of physiology at the University of Cambridge, will lecture on animal physiology at the Iowa State College of Agriculture and Mechanic Arts during the first term of the summer session of 1932.

DR. M. BODANSKY, director of the John Sealy Memorial Laboratory, Galveston, and professor of pathological chemistry at the University of Texas School of Medicine, will be visiting professor of biological chemistry at the American University of Beirut, Syria, during the fall semester of 1932–33.

DR. ANTON J. CARLSON, of the University of Chicago, gave a lecture on the "Rôle of the Nervous System in Gastro-intestinal Functions and Disorders" before the Los Angeles County Medical Association on December 22; on the previous day Dr. Carlson spoke on "The Present Status of the Problem of Endocrines" before the Clinical and Pathological Society.

PROFESSOR G. W. SNEDECOR, of the department of mathematics of the Iowa State College, gave from January 4 to 7 five lectures on "The Analysis and Interpretation of Research Data" before members of the faculty and others engaged in research work at the Louisiana State University.

IN the winter and spring Professor Thomas R. Garth, of the department of psychology at the University of Denver, has engaged to give a lecture on "Race Psychology" at various institutions in the Middle West and in the East. His itinerary will include the University of Iowa, Wellesley College, Smith College, the New School of Social Science, New York City; Davidson College, North Carolina, and Washington University, St. Louis.

THE patent obtained by Dr. Edward J. Dyce, of Cornell University, for a new product called crystal honey has been turned over to the university for use in the advancement of the honey industry.

THE Committee on Scientific Research of the American Medical Association invites applications for grants of money to aid in research on problems bearing more or less directly on clinical medicine. Preference is given to requests for moderate amounts to meet specific needs. Application forms may be obtained from the Committee at 535 North Dearborn Street, Chicago, Illinois.

AT a meeting of the executive committee of the Southwestern Division of the American Association for the Advancement of Science at Tucson on December 21, it was voted to hold the coming spring meeting of the division at Denver, April 25 to 28, inclusive, under the presidency of Dr. John D. Clark, professor of chemistry and dean of men at the University of New Mexico. On the general program will be the annual John Wesley Powell Memorial Lecture and a symposium on readjustments in the light of changed economic conditions. Scientific organizations in the territory of the Southwestern Division are especially invited to participate in the meeting. On account of the sabbatical leave of Dr. Edwin F. Carpenter, the secretary of the division, Dr. M. M. R. Schneck, of the department of psychology, University of Arizona, was appointed acting secretary until September, 1932.

AT the recent meeting of the executive committee of the American Society of Zoologists in New Orleans, it was voted to hold the next annual meeting in Atlantic City, New Jersey, from December 27 to December 29, in affiliation with the American Association for the Advancement of Science.

IT was announced at the close of the Baltimore ses-

sion that the Society of American Bacteriologists will hold their next annual meeting at the University of Michigan.

ON the invitation of the secretary of the Smithsonian Institution, Dr. Charles G. Abbot, and of the assistant secretary in charge of the U. S. National Museum, Dr. Alexander Wetmore, the third annual meeting of the American Association of Physical Anthropologists will be held at the U. S. National Museum, March 21, 22 and 23. This meeting will give opportunity to see or review the extensive collections of anthropological and comparative material acquired by the National Museum. For the benefit of more distant members who belong to the American Association of Anatomists also, the date of the meeting has been arranged to facilitate attendance at both conventions, the anatomists meeting in New York on the three succeeding days.

A CORRESPONDENT writes: "Over five hundred members have already enrolled for the sixth international Congress of Genetics. Many of these have done so recently. Because of the past uncertainty of holding the congress, it seems probable that there are many other individuals who, now that it has been definitely decided to go ahead, will wish to join as soon as possible. For this reason the council has decided to extend the date for ten dollar memberships until May 1. After that date, the membership will be at an increased fee ($12.00). Graduate students and assistants may join as six-dollar members until May 1. After that date their memberships will be at the rate of seven dollars. Both of these revisions have been made because of the exceptional economic conditions and because the council is anxious, in so far as it is possible, to avoid having financial considerations a deterrent to memberships. The number of people who have already joined insures a representative and highly successful congress, and reveals as well the great amount of active interest in genetics at the present time. The guarantee that full proceedings will be published and that each member will receive a copy has proved to be a strong attraction, not only to geneticists but to many biologists in other fields."

MRS. HENRY LANG has presented to the Montclair Art Museum in memory of her mother, the late wife of Jasper R. Rand, who died in 1909, an addition, built at the cost of $100,000, devoted to an exhibition of American Indian material.

DISCUSSION

DEFINITION OF A MATHEMATICAL GROUP

IT is just about a century ago that the word group began to be used as a mathematical term by a young Frenchman named Evariste Galois, who died before he reached the age of twenty-one, but who had at that early age inaugurated such fundamental work that he is still ranked among the 25 greatest mathematicians that ever lived and the only one among them who gained this eminence by work accomplished at such an early age. Although the concept of group had been employed occasionally in mathematics before the time of Galois his new uses thereof were probably the most influential in the vast developments relating thereto, which took place during the hundred years just closing. It is well known that these developments are now extending into such subjects as mathematical physics, where they have sometimes been called a pest, and also into elementary mathematics, as may be seen from the fifty-one pages devoted thereto in the recently published volume on analysis of the "Enciclopedia delle Matematiche Elementari."

It may be appropriate to note here in connection with the termination of the first century of the use of the mathematical term group a few elementary notions relating to its definition without entering into technical details relating thereto, especially since widely different definitions of this term appear in standard works of reference. It seems to have been always assumed therein that the distinct elements of a group satisfy the two conditions that they include the product of every pair as well as the square of every one of them. In many cases the elements are such that no other condition is necessary in order that they must constitute a group even in the most restricted sense of this term. This is true, for instance, when these elements are the permutations with respect to which groups were first studied by J. L. Lagrange, P. Ruffini, E. Galois and others. It may therefore be of interest to note here the circumstances under which one of these two conditions implies the other as regards a given set of such elements.

It is easy to see that the latter of these two conditions implies the former only in the trivial cases when the group is either the identity or of order 2. That is in every other group it is possible to find a set of distinct generating elements so that the set includes the square of each one of these elements but not the product of every pair thereof. On the contrary, the former of these two conditions implies the latter with very few elementary exceptions. One such exception is furnished by the set composed of the identity and the six elements of order 4 of the quaternion group. It is easy to verify that the set of seven elements thus obtained includes the product of every possible pair

of its distinct elements but not the square of its elements of order 4. Since the quaternion group plays such an important rôle in the history of mathematics it is interesting to note that it is the only non-abelian group which involves a set of generating elements such that they satisfy the former of the two given conditions without implying the latter.

Among the abelian groups it is obvious that every cyclic group contains such a set of two operators composed of a generator of the group and the identity. When the order of a cyclic group exceeds 2 it also contains such a set of three elements composed of a generator, its inverse and the identity. When a group involves only elements of order 2 besides the identity, all its elements of this order constitute such a set. As these elementary groups are the only ones which separately involve such a set it results that when a set of distinct group elements satisfies the conditions that it includes the product of every pair of these elements and does not generate one of these elementary groups it must also involve the square of each of its elements and hence constitute a group. Since the properties noted above lie at the very door of group theory they should be of interest not only to those who desire to enter therein but also to those who wish to know only where this subject abides and whence its growing influence emanates.

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AUTHORS' ABSTRACTS[1]

DR. LAMSON, and other readers of SCIENCE, may be interested to know, in connection with Dr. Lamson's article on *Biological Abstracts* in SCIENCE for November 13, that the principle of having authors' abstracts accompany all geological publications has been accepted by geologists.

A motion asking that the International Geological Congress recommend such abstracts was presented, by a representative of the division of geology and geography of the National Research Council, at the fifteenth session of the congress, which met in South Africa in 1929, and was passed by the congress. A year or two before that, authors' abstracts were called for in all publications of the Geologic Branch of the U. S. Geological Survey, and a committee of the branch prepared a set of suggestions for authors of abstracts. The plan of having authors' abstracts in the publications of the various state geological surveys has also been presented to the state geologists in their annual meeting and approved by them. A memorandum calling for authors' abstracts in publications of all branches of the U. S. Geological Survey has just been issued. In two geological publications, the *Journal of Geology* and the *Bulletin of the American Association of Petroleum Geologists*, authors' abstracts accompany almost all papers. Authors' abstracts of all papers to be presented at the annual meeting of the Geological Society of America are distributed before the meeting and printed in the annual volume.

As far as actual accomplishment goes, these results are still far from covering the field of geological publications. The action of the International Geological Congress is not binding in any way; it is merely a very valuable step in the promotion of the project. It has, however, created interest in the idea in other countries and led to its adoption by a number of important publications.

That ultimately the idea will be generally accepted in scientific publications can scarcely be doubted. The additional percentage of time required for an author to prepare an abstract of a paper which he has just completed is so slight that the only objection there can be to it is the additional bother; but when authors generally have fully realized the growing impossibility of keeping up with the flow of scientific literature, the unfairness of burdening their more public-spirited colleagues with the preparation of an abstract of another's paper, and the value to themselves of an abstract as a means of bringing before their busy colleagues the essence of their publication, they will be not only willing, but eager, to prepare the best possible abstract.

Biology and chemistry have perhaps solved, even under present conditions, the problem of covering with abstracts the entire field of their interests. But it is hard to believe that when those more public-spirited members of these professions, who prepare the abstracts, once realize the needlessness of their self-sacrifice, they will submit indefinitely to being made the goats. It is true that not all authors, at present, know how to prepare good abstracts of their own papers. Some authors find it hard to write an abstract much shorter than the original paper; but a study of abstracts indicates that the majority of authors' abstracts are too short and too incomplete. Either type of defect may be due to insufficient effort as well as to inexperience. But it is worth while to point out another peculiar psychological twist which leads to inadequate abstracts; that is, that some authors, in preparing abstracts, have in mind, often unconsciously, to conceal from the reader as far as possible the conclusions arrived at and, by merely indicating the subject-matter with which the paper deals, to lead the reader of their abstract to read the paper itself. If they knew how often this ruse fails they would probably soon abandon it.

Under paragraphs (1) and (2) of his discussion of the possibilities of carrying out the plan of having

[1] Published with permission of the Director of the U. S. Geological Survey.

authors' abstracts with all biological publications, Dr. Lamson takes up the question of obtaining the co-operation of editors of biological journals and of their contributors. At first glance it would seem that so simple and reasonable a plan would obtain the immediate support of all editors. On investigation, however, a reason, and probably the only one, for hesitation on the part of some of them becomes obvious. Many journals are dependent on their subscribers for their existence. They must, therefore, obtain as many interesting papers as they can, and some of them hesitate to make any special demands on possible contributors, that might lead the contributors to offer their papers elsewhere. This difficulty, as Dr. Lamson points out, will diminish as the requirement of authors' abstracts becomes more general with the stronger, more independent publications and as authors awaken to the reasonableness and advantage of preparing abstracts of their own papers. When the expense in cash, in wasted time, and in the duplication of researches through ignorance of what has gone before (of which there are some astounding examples) is considered, it would undoubtedly be to the economic advantage, not only of science but of the community in general, to promote the preparation of authors' abstracts by a subsidy to those journals which require them of their contributors. However, time, good sense and fair-mindedness will undoubtedly bring about the same result.

MARCUS I. GOLDMAN

U. S. GEOLOGICAL SURVEY

THE DISTRIBUTION OF STATE AGRICULTURAL EXPERIMENT STATION BULLETINS TO FOREIGN COUNTRIES[1]

IT is recognized generally that the character of the work of the state agricultural experiment stations has undergone a gradual change during the past twenty-five or thirty years. In recent years a much larger proportion of the bulletins published are technical in nature. Many of them constitute distinct contributions to science and are of interest to agricultural workers throughout the world. If these bulletins are to justify the labor and expense involved in producing them they must be so distributed that they will be readily available to investigators in the field of agricultural science in all countries. A personal inspection of the libraries of a considerable number of European institutions of agricultural research led one of the writers to suspect that such adequate distribution was not being accomplished. A further investigation of the extent of the distribution of bulletins to foreign libraries was made by sending a questionnaire to fifty state agricultural experiment stations. The earlier suspicion was thoroughly confirmed. The agencies for distribution have not kept pace with the change in the nature of bulletins to be distributed. Distribution to libraries in the United States is adequate, but only a few states have built up a mailing list of foreign institutions of any appreciable size. Many of the foreign mailing lists consist largely of the names of individuals who have requested that their names be placed on the mailing list. Bulletins so distributed into the hands of individual workers doubtless serve a useful purpose, but it is obvious that the first responsibility of distribution is to see that a copy of the bulletin is deposited in the library of those institutions where agricultural research or teaching is in progress.

The task of securing adequate distribution of these bulletins need not be done entirely with altruistic motives. Many people familiar with the lavish way in which agricultural experiment station bulletins are distributed within the United States do not realize that nearly all research institutions in European countries make a charge for their publications and do not send them gratuitously, even to other institutions, unless exchange relations have been established. Most foreign institutions are glad to exchange publications with our state experiment stations, but are generally quite careful to see that a real exchange basis is reached. If our state experiment stations wish to receive the research publications of foreign institutions, it behooves them to see that these institutions receive their bulletins.

With help from various sources the writers have prepared for the Minnesota Agricultural Experiment Station a classified list of libraries of foreign agricultural institutions. Approximately eleven hundred libraries are included. About six hundred of these will receive all bulletins published by the experiment station. The remaining addresses are classified according to subject-matter interest and will receive those bulletins in which they are interested. The list is by no means perfect, but considerable time has been spent in an effort to make it complete, at the same time weeding out those institutions with only a superficial interest in agricultural research. It is realized that constant revision of such a list will be necessary if it is to function to the best advantage.

Attention is called to this list and to the present inadequate distribution of bulletins to foreign institutions in the hope that other states may take steps to secure better distribution of their bulletins. It is the opinion of the writers that some central agency, by proper cooperation with all the various state experiment stations, could render a distinct service to

[1] Paper No. 1061 of the Journal Series of the Minnesota Agricultural Experiment Station.

agricultural science in bringing about more efficient exchange of research publications.

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AUDITORY ACTION CURRENTS

IN a recent issue of SCIENCE and in subsequent publications[1], Wever and Bray have reported that action currents which were led off from the acoustic nerve and amplified were heard in a telephone as tones of the same pitch as the stimuli, a result obviously of great importance for auditory theory. Since then Adrian, and Davis and Saul have in brief notes reported the repetition of the results, although Adrian has not agreed in attributing the currents to the auditory nerve. We, too, have repeated these experiments under conditions which Wever and Bray specify as adequate, but when certain sources of error are excluded we have obtained only negative results.

In most of our experiments we have used a 5 stage amplifier, 3 stages resistance-capacity coupled and 2 power stages, transformer coupled. Tests showed that changes 20 μ V in the input circuit could be distinctly heard in the telephone. (One should like to know whether the amplification figures given by Wever and Bray for their apparatus refer to voltage or power amplification, and whether they represent the theoretical or measured amplification.) In our experiments we have used both metal hooks and cotton thread electrodes; have led off from auditory nerve, brain stem, inner auditory meatus and round window; have connected electrodes to amplifier over transformer and at other times directly to grid and filament; have used both decerebrated and undecerebrated cats—but always with negative results.

Our experiments demonstrated, however, the possibility of certain purely physical causes for Wever and Bray's results which seem not to have been excluded by their checks. They report having set up a telephone line between operating room and observation room and that, for purposes of comparison, the sound stimuli were introduced alternately into the transmitter and into the cat's ear. If this is done, unless the transmitter circuit is definitely broken when sounds are introduced into the cat's ear, it is obvious that the currents in the transmitter circuit can induce upon the input circuit of the amplifying system, as in the familiar cross talk between telephone lines. Wever and Bray's steps to exclude induction as a source of error refer only to the induction possible between the sources of sound stimuli and the electrodes, but not to the type just mentioned. The mere provision of screening does not assure the exclusion of all electric and magnetic induction effects. Screening might in some cases favor a coupling of the telephone and amplifying systems instead of hindering it.

Nor is the possibility of such sources of error necessarily excluded by the physiological checks performed by Wever and Bray. The primary, input circuit of their amplifier consists in part of the animal tissues between the electrodes. Changes in the electrical properties of the tissues as a result of various physiological changes, such as the restriction of the blood supply, death of the animal and destructions of various sorts, would also change the receptivity of the circuit for induced currents. So, for example, if the induced currents in the input circuit fall beneath a certain minimum as a result of increased impedance of the animal tissues, then these currents can not be transmitted over the transformer for amplification, and the sound signals would no longer be heard in the telephone.

Induction effects of the sort mentioned are not dependent upon the presence of a transmitter system as used by Wever and Bray. Other electrical instruments in which the sound stimuli can occasion currents or modifications of current which can in turn induce upon the amplifying system may also cause such errors. In many commercial and house telephone systems, the receiver circuit is closed, even when the transmitter is hung up. The telephone receiver, because of the magnetic field always present, can of course act as a transmitter and produce variations of current that may induce corresponding variations in the amplifier. Similar effects might be produced even by a loud-speaker or mirror galvanometer that happened to be standing nearby.

We believe that the possibility of such sources of error must be definitely excluded before the Wever and Bray effect can be conclusively attributed to action currents of the acoustic nerve.

GEORGE KREEZER
HANS DARGE

BERLIN, GERMANY,
OCTOBER 13, 1931

[1] E. G. Wever and C. W. Bray, "The Nature of Acoustic Response," *J. Exper. Psychol.*, 13, 373–387, 1930; E. G. Wever, "Impulses from the Acoustic Nerve of the Guinea Pig, Rabbit and Rat," *Amer. J. Psychol.*, 43, 457–462, 1931; SCIENCE, 71, 215, 1930.

THALLIUM POISONING AND SOIL FERTILITY

THALLIUM sulfate has potential destructive effects on vegetation which have not received adequate attention from those advocating its use in vermin control. It probably is not the intention of the Bureau of Biological Survey and other agencies practicing vermin

control to bring about the enduring sterility of the soil upon which thallium sulfate baits have been used. But there is real danger that the practice will lead to such an end, as will be evident from the observation here noted.

Thallium sulfate baits were suspended on stakes at a height of a few inches above the earth on a grassy slope in the arboretum of the Hawaiian Sugar Planters Association Experiment Station.[1] This lies in the head of the Manoa Valley near Honolulu, a region of heavy rainfall. The baits, each containing about 0.5 gm of Tl_2SO_4,[2] were set out in July, 1929. At the time of my inspection, July, 1931, there was below each stake a patch of bare earth, from one to three feet long and perhaps a third as wide, upon which all vegetation was killed soon after the baits were set out. *Not a single trace of plant life was or had been apparent on these areas during the intervening two years.* While no exact records have been made, there appeared to have been no diminution in the size of these denuded areas.

Thallium sulfate scattered or placed as bait ultimately reaches the soil, whether the bait is taken or not. Under our continental conditions, where rainfall is much less than at the locality here cited, thallium sulfate would presumably be even less rapidly removed from the soil by leaching. Continued scattering of thallium sulfate baits will presumably lead to:

(a) Complete denudation of numerous small areas of pasture or range land, leading to reduction of plant yield and also perhaps to erosion or blow-hole formation.

(b) General toxicity or complete sterility of cultivated land, which may be expected to destroy its agricultural value completely for an unknown period.

Any group or organization which allows or urges the extensive use of measures entailing the possibility, even, of such results incurs grave responsibility; and any owner of agriculturally valuable land should look well to his own future before allowing thallium compounds to be spread upon his soil.

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[1] The writer is indebted to Mr. E. L. Caum, of the Hawaiian Sugar Planters Association Experiment Station, for the invitation to visit the arboretum.

[2] This corresponds very closely to the amount of Tl_2SO_4 in "a handful" of barley treated with 1 lb. of Tl_2SO_4 per 100 lbs. of grain. Dilling has reported (*Ann. Appl. Biol.*, 13: 160–167, 1926) that N/600 Tl_2SO_4 completely and irrevocably prevents the germination of seeds of *Lepidium sativum*. More dilute solutions were not studied. On this bases it might be guessed that one bait, containing 0.5 gm of Tl_2SO_4, would under average conditions sterilize not less than half a cubic foot of soil. The observations here recorded seem to suggest a slightly greater effectiveness.

ESSENTIAL FATTY ACIDS AND GOITER PRODUCING SUBSTANCES[1]

Two recent papers indicate that the writers have not yet come to a realization of the importance of the question of *iodin-fat* balance which was raised by R. McCarrison[2] and the Mellanbys,[3] but has apparently been forgotten by those authorities as well.

G. O. Burr, M. M. Burr, and W. R. Brown,[4] studying the nutritive value of cod liver oil, find that the feeding of cod liver oil does not prevent scaliness of the feet and tail in rats given their fat-free diet. They state that of the more unsaturated oils, cod liver oil is distinct in that it leaves the skin of the animals scaly, while lard, olive oil, corn oil, linseed oil, methyl linolate and methyl linolenate all produce skin free from scales and dandruff. They emphasize the fact that cod liver oil is highly unsaturated without linoleic and linolenic acids being present in appreciable quantities. Burr and Burr for some time contended that linoleic and linolenic acids must be furnished as a part of the diet in their fat deficiency experiments.

The findings of the writer and associates[5] indicate clearly that the reason why cod liver oil is ineffective in Burr's fat disease is on account of the iodin carried with it. Burr's animals, long depleted of fats and at the same time receiving KI, must be given fats lacking iodin, or the iodin-fat balance will not be restored. The significance of Burr's essential fatty acids[6] depends on their *degree* of *unsaturation* and not upon any specificity.

E. J. Bauman, Anna Cipra, and David Marine,[7] in a paper just published, have shown that goiter-producing substance in cabbage was extractive with ether from "cabbage fat." They failed to refer to the important pioneer work of McCarrison and the Mellanbys, to our own recent work and to the suggestion that we advanced[8] to the effect that their "goiter producing substance" was quite probably the *unsaturated fat* of cabbage from which they had permitted a loss of iodin.

F. E. CHIDESTER

[1] From the Laboratories of West Virginia University, Morgantown, West Virginia. Aided by a grant from the National Research Council.

[2] *Ind. Jour. Med. Res.*, 7, 633, 1919–1920.

[3] E. and M. Mellanby, *J. Physiol.*, 55, vii–viii, 1921.

[4] *Proc. Soc. Exp. Biol. and Med.*, 28, 905, 1931.

[5] SCIENCE, 68, 42, 1928; Collecting Net, 5, Nos. 32 and 33, 1930; *Proc. Soc. Exp. Biol. and Med.*, 28, 187–189, 1930; *Anat. Rec.*, 47, 3, 304, 1930; *Med. Times and L. I. Med. Jour.*, 59, 138–9, 1931.

[6] G. O. Burr and M. M. Burr, *J. Biol. Chem.*, 82, 345, 1929; G. O. Burr and M. M. Burr, *J. Biol. Chem.*, 86, 587, 1930.

[7] *Proc. Soc. Exp. Biol. and Med.*, 28, 9, 1017–18, 1931.

[8] Chidester and Wesson, *Med. Times*, 58, 11, 319–321, 1930.

THE DISCOVERY OF THE OESTRUS CYCLE IN MAN AND OTHER MAMMALS

THE cyclical changes which take place in the uterus and the vaginal mucosa of mammals have been studied in detail within the last few years. This work received its principal impetus from the reports of Stockard and Papanicolaou[1] and of Long and Evans[2], both of which are dealing with the rat. Since then various other mammals have been investigated and Dierck[3] and others have discussed the phenomena of the oestrus cycle in the human female. The importance of this work is generally acknowledged. It should, therefore, be of some interest to call attention to early work in this field which seems to be but little known. F. A. Pouchet, in his work, "Théorie positive de l'ovulation spontanée et de la fécondation des mammifères et de l'espèce humaine, basée sur l'observation de toute la série animale," published in Paris in 1847, gave a rather detailed account illustrated by three plates, of observations which he had made upon smears from the uterus and vagina of human females and sows. He definitely recognized the regular cyclical character and the physiological importance of these phenomena. The only reference to Pouchet's book which the writer has seen in the recent literature concerning the oestrus cycle was found in the monograph by Long and Evans. Since these authors, however, refer to Pouchet only as authority for spontaneous ovulation, and since they state that the first description of changes in the vaginal mucosa was given by Morau in 1889, it may be inferred that they had not seen Pouchet's original book.

WALTER LANDAUER

STORRS, CONN.

CHROMOSOME NUMBERS IN ULMUS

THE first report on chromosome number in this genus was by Krause in 1929. He reported fourteen chromosomes as the haploid number for *Ulmus montana* With. In some unpublished work Krause has reported that the haploid number of chromosomes for *Ulmus americana* L. is 14 and for *U. campestris* L. is 14+.

Flower buds of *Ulmus pumila* L., *U. fulva* Michx., and *U. americana* L. were collected during the spring of 1931. Belling's aceto-carmine method was used to determine the stage in development of the buds at which division figures appeared. Fixations were made of such material, and chromosome counts were made from polar views of equatorial plates of both heterotypic and homeotypic divisions. It appears that the haploid chromosome number of *U. pumila* and of *U. fulva* is fifteen and that that of *U. americana* is twenty-eight or thirty. The apparent variation in the case of *U. americana* may possibly represent a difference between individual trees.

A study of the chromosomes in somatic cells is planned as well as a more detailed study of the chromosomes during meiosis.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

PHOTOMICROGRAPHY WITH A VEST POCKET CAMERA

THE method of adapting a box camera for photomicrography given in a recent issue of SCIENCE[1] by Mr. Apgar suggests a simpler solution of the problem which I used some time ago.[2] A vest pocket kodak can be used to make photomicrographs by setting it over the ocular of the microscope without additional focusing apparatus. The series with the f. 6.9 lens in a focusing mount fits the ocular very well, as the rolled edge of the microscope ocular makes a light-tight connection with the lens mounting, and the camera is so balanced that no additional support is needed. The manufacture of this very useful kodak has been discontinued, but it may be obtained for a nominal sum from dealers having second-hand kodaks. Other cameras of similar size may also be used.

When the draw tube of the microscope is extended to the proper distance of 160 mm (Leitz 170 mm) and the microscope focused sharply with the normal eye or with an eye properly fitted with spectacles a sharp picture may be obtained when the camera lens is set at infinity. If the draw tube is not extended to the proper distance a minus lens of the proper focal length must be placed over the eyepiece when the microscope is focused but removed before the camera is placed on the microscope.[3] Having the draw tube closed prevents any slipping due to the weight of the camera and does not greatly reduce the definition of the microscope. With one microscope that I used in this way, a –9 dioptera lens gave the necessary correction. The actual amount of correction needed will depend on the instrument used, so the above

[1] *American Journal of Anatomy*, Vol. 22, 1917.
[2] "Memoirs of the University of California," Vol. 6, 1922.
[3] Archiv für Gynäkologie, Vol. 130, 1927.

[1] C. E. Apgar, SCIENCE, 74: 487–8, 1931.
[2] O. W. Richards, *Bot. Gaz.*, 86: 93–101, 1928.
[3] This suggestion comes from Foot and Strobel, quoted by M. F. Guyer in "Animal Micrology," p. 150, Chicago, 1921.

figures merely indicates the approximate magnitude of required lens.

It is preferable to use the microscope at the proper tube length for the greatest definition, and this can be done if a rubber ring cut from a piece of rubber hose is placed around the draw tube to prevent telescoping from the weight of the camera. Many modern microscopes have the draw tube fixed at the proper length, so no additional friction is needed.

The use of the vest pocket kodak with the autographic feature allows permanent labeling of the negatives, which can not be done with the box camera. The magnification obtained will depend on the lens on the camera and the distance from the lens to the film. It can be obtained easily by photographing a stage micrometer. When it is desirable to reduce the cost of the negative film an insert can be made to reduce the opening in the back of the camera, cut from the thin aluminum of an ordinary cookie tin, and less expensive moving picture film used. The distance that the film is to be turned ahead between pictures can be noted by extending the opening on the back of the camera to the edge of the film and counting the perforations of the film through a red window as the film is wound. The disadvantage of this method is that the camera must be loaded and unloaded in the dark room or in a changing bag. In the previous work[2] I used an insert with an opening of 1 x 1½", which is twice the size of the cinema frame and thereby reduced the cost of the negatives to about one cent per exposure.

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A NEW TECHNIQUE FOR OBTAINING OOSPORES OF THE HOP DOWNY MILDEW BY INOCULATING COTYLEDONS

FIELD observations in the Fraser Valley, British Columbia, in 1930, showed that young hop seedlings were badly infected with the hop downy mildew, *Pseudoperonospora Humuli* (Miy. and Tak.) Wilson. Cotyledons and primary leaves were covered with conidiophores bearing conidia.

Following this observation hop seed were gathered from plants of the clusters variety and sown in flats in the laboratory in 1931. As the cotyledons and young primary leaves appeared, they were inoculated by placing on them minute portions of infected leaves obtained from diseased "basal spikes." The seedlings had been previously moistened. The seedlings were afterwards covered with vials so as to maintain maximum humidity. They were grown in the basement of the laboratory, where the temperature remained fairly constant at 58° to 65° F. The seedlings were moistened with water each day.

After a period of six days, the time varying with different seedlings, it was noticed that the cotyledons showed signs of "damping off." Microscopic examination of the cotyledons showed that no conidiophores had developed, but on teasing out the tissue, it was found that oogonia and oospores were present in abundance. Approximately eighty oospores were found in each cotyledon, giving an average of 160 oospores per seedling. The oospore dimensions corresponded with those reported by other workers, ranging between 23 and 37 μ. Further work revealed that when maximum humidity was not maintained, conidiophores bearing conidia were produced, as well as oospores.

At present it is possible to collect abundant oospore material by inoculating hop seedling cotyledons in the manner described and gathering the latter when they show signs of damping off. By this method it will also be possible to obtain conidia for experimental purposes in the greenhouse during the winter period.

The writer considers that this technique of inoculating cotyledons may be applicable to other members of the Phycomycetes, which means a great saving of time for the worker who wishes to examine oospore material within the host.

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SPECIAL ARTICLES

ON THE VARIATION OF THE OXYGEN CONTENT OF CULTURAL SOLUTIONS

EARLIER observations led to the conclusion that there may be a translocation of oxygen from the shoot to the root of plants when the shoot is in sunlight and the root is in soil.[1] The present note records observations, to be published in detail elsewhere, that a similar movement of oxygen may take place when the roots of plants are in cultural solutions, particularly in distilled water.

The plants referred to are willow, cotton, corn and sunflower. These were grown in a standard culture solution and were transferred to distilled water for

[1] W. A. Cannon, "Studies on Roots," Carnegie Institution of Washington. Year Book No. 25, p. 317, 1925–26.

The investigation was carried on in part with the aid of a grant from the American Association for the Advancement of Science.

the tests only but were kept ordinarily in the culture solution. One willow cutting, or three of the plants, were placed in each glass jar, darkened always and of one-quart size. The cultures were subjected to two- or three-hour periods, alternately, of sunlight and of dense shade, and were covered with bell glasses over which a spray of water was constantly played. By this means the temperature both of the solutions and of the bell glasses was kept fairly low and constant, and, also, the atmosphere of the bell glasses was maintained highly humid. Fresh distilled water was used in each period.

The series of experiments of August 12 to 21 on the sunflower will serve to illustrate the leading results obtained by a study of all the plants, except only corn, which will be referred to below.

In these experiments two periods were selected each day for the tests. Of these one ran from 8 to 11 A. M. and the other from 11 A. M. to 2 P. M. Except in one instance the plants were kept in shade during the first of the periods and were exposed directly to sunlight during the second. The temperatures of the bell glasses and solutions were recorded in each test, and, at the same time, the amount of oxygen was determined. The results may be briefly given. In every instance more oxygen was found in the cultures exposed to sunlight than in those kept in shade. The difference was about 10 per cent.

In further account of the experiments it should be stated that the temperature of the bell glasses in the sunlight, and of the solutions as well, was higher than that of the solutions and bell glasses in the shade. As the rate of respiration, at the temperatures used, varies directly with the temperature, it would be expected that where the temperature was low the oxygen content would be high, and *vice versa,* but the opposite was found to be the case. That is to say, the greatest amount of oxygen was in the solutions with the highest temperatures. But these were also in sunlight, or at least the plants growing in them were in direct sunlight. The conclusions, therefore, seem warranted that there is an important relation, even if an indirect one, between evolution of oxygen in photosynthesis and the variation of the oxygen content of the solution as noted above. Thus it appears that oxygen, liberated in the course of carbon dioxide assimilation in chlorophyll bearing organs and in light, moves downward toward and ultimately into the root to fill wholly or in part its requirements for oxygen. Thus the root may apparently secure its oxygen supply from one of two sources: from the plant itself, internal oxygen,[2] and from the immediate environment. Whether this is the case under "natural" conditions, as it has been found to be in the experiments, remains to be seen. However that may be, that oxygen taking its origin in this manner constitutes a large proportion of what the root may ordinarily use seems quite possible, and under conditions similar to those employed in the experiments reported, it is more than probable.

It has already been mentioned that corn was found to be an exception to the effect, although indirect, of sunlight on the oxygen content of solutions similar to those in which the sunflower, cotton and willow were growing. To show this it will suffice to summarize one representative series of tests. In this series in which the temperature varied in the same direction as that in the experiments on sunflower above cited, but in which the variation was not so great, the oxygen concentration of the cultures in sunlight was always less than the concentration in the shade tests. It was found, in short, that the roots of plants exposed to sunlight absorbed about 14 per cent. more oxygen than did those of shaded specimens. This suggests either that the rate of respiration of the roots of the plants which had been in sunlight was high because of the higher temperature, or more rapid than the supply of internal oxygen from the shoot, or perhaps that the latter was wanting. However this may be, it can be pointed out that at temperatures favorable for a rapid rate of root growth corn appears to require an exceptionally good supply of oxygen.[3] Whether, on the other hand, such a form as willow, which does not appear to have a high oxygen requirement for root growth, may depend importantly on the shoot for its oxygen supply is another matter but may well be the case. It either is in solution or is a gas. In whatever state or in whatever way the internal oxygen may move from one part of the plant to another, whether molar or molecular, or both, presents no especial difficulty. In the event that it is tied up with the movements of solutions it yet can make its way from tissue to tissue and from the shoot to the root. Downward movement of solutions in plants are well known. This may even extend to the root where, as in the present study, movement of a solution from the leaves to and well into the roots of sunflowers was demonstrated to take place. It thus appears possible that internal oxygen may take an important part in the aeration of plant tissues, whether they are of the shoot or of the root, although whether this is of general occurrence among land plants must remain for further investigation.

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[2] G. F. Beardsley, and W. A. Cannon, "Note on the Effects of a Mud-flow at Mt. Shasta on the Vegetation," *Ecology,* 11, 1930.

[3] W. A. Cannon, "Physiological Features of Roots," Pub. No. 368, Carnegie Inst. Washington, 1925.

A DIAPORTHE CANKER OF AMERICAN ELM

WITHIN the past few years specimens of American elm affected by a canker disease were collected from two different localities in eastern Massachusetts. Specimens from one locality consisted of dead branches, bearing large conspicuous cankers which in many cases had girdled the branches. Many of the diseased areas were hypertrophied, with margins of thick cork layers surrounding areas of exposed wood. In some cases the branches appeared to be twisted because of the irregular semi-spiral arrangement of several contiguous and much calloused cankers. The other specimens bore cankers in a younger stage of development partially girdling the branches. The diseased areas were slightly depressed, roughened and cracked along the margin. The surface of the cankers was covered with small pustules. On one of the branches the canker extended through several nodes and was sharply delimited by a definitely raised layer of cork. Branching out from this diseased area was a young twig, which had been killed back. The new growth was stunted and the leaves had become brown and dry before reaching maturity.

The pycnidia on the host are solitary, black, smooth, carbonous, ostiolate with short necks, conical or elliptical. The pycnidial cavity is irregularly shaped because of the protrusions of the basal layers. No compound pycnidia were observed in any sections. The spores formed in the pycnidia are of two types, the alpha spores and the beta spores or so-called stylospores. Both the alpha and beta spores are produced on the same type of sporophore, which is subulate or clavate, persistent, and measures approximately 11.2–15.2 μ x 2.8–5.2 μ. The alpha spores on the host and from the twig cultures are hyaline, one-celled, biguttulate, ovoid or elliptical with subacute extremities but occasionally with narrow pointed ends. The standard range of one hundred spores from the host was 6.5–8.2 μ x 2.7–3.5 μ, while that of one hundred spores from twig cultures was 7.1–8.8 μ x 3.1–4 μ. The beta spores are long, cylindrical, hyaline, one-celled, usually hamate and tapering to a point at the curved end. The standard range of one hundred spores was 22.7–27.5 μ x .98–1.3 μ.

The perithecia were obtained in cultures on sterilized twigs and on malt agar. On the twigs the perithecial stage is indicated by the appearance of protruding corrugated black beaks. The perithecia are single or clustered, always separately erumpent, membranous, leathery, globose, and measure approximately 400 μ x 385 μ. They are usually in darkened effuse stromatic areas, and beneath these dark marginal zones lines develop in the wood. The beaks are elongate, slender, projecting about 5 mm from the surface of the twig. The interior of the necks is lined with periphyses. The asci are cylindrical, clavate, and measure approximately 33–50 μ x 5.1–6.9 μ. At the apex of the ascus the wall is much thickened and the pore is surrounded by a refractive ring. Paraphyses are usually present; they are long, slender, sinuous, continuous, simple, without bulbous tips, and with granular contents. The ascospores are uniseriate or biseriate, two-celled, hyaline, slightly constricted at the septum, four-guttulate, granular, with a standard range of 10.9–12.3 μ x 3.8–4.5 μ for one hundred spores.

Naturally it was of interest to determine if possible the exact identity of this fungus, which apparently caused a pronounced canker disease of American elm. According to the description given by Gaüman[1] following Von Höhnel's classification, the ascospore stage should be placed in the rearranged family Diaporthaceae because of the sessile asci which break away from the hymenial layer at maturity. The fungus seems to correspond with the genus Diaporthe as described by Wehmeyer[2], since it has hyaline, ellipsoid, two-celled spores and forms a dark marginal zone line in the wood. The conidial stage of the fungus studied also seems to correspond with the form genus Phomopsis reported by Wehmeyer. The following five species of Diaporthe described by Saccardo[3] were carefully compared with the fungus studied: *D. protracta* Nke., *D. perjuncta* Niessl., *D. discutiens* Sacc., *D. Malbranchei* Sacc., *D. eres* Nke. The description of *D. eres,* however, corresponds most closely. The descriptions of species of Phomopsis on elm are so fragmentary that a satisfactory comparison of species is extremely difficult. However, seven species described by Saccardo have been compared with the Phomopsis now being studied. *Phomopsis oblonga* Sacc. has been most completely described and appears to resemble this Phomopsis. Saccardo considers *Ph. oblonga* with alpha spores 6–7 μ x 3 μ and beta spores 33 x 1 μ to be the imperfect stage of *Diaporthe eres.* Grove[4] on the other hand reports *Ph. eres* Sacc. with alpha spores 9–10 μ x 2.5–3 μ and beta spores 25–30 μ x 1 μ as the imperfect stage of *D. eres.* Grove mentions that Saccardo has suggested that *Ph. oblonga* and *Ph. eres* are the same, although Saccardo gives distinctly different spore measurements for each. It is impossible to state the exact identity of either the Diaporthe or the Phomopsis of the present study, since type specimens were not available for comparison.

[1] E. Gaüman, "Vergleichende Morphologie der Pilze." Jena, 1926.

[2] L. E. Wehmeyer, "A Biologic and Phylogenetic Study of the Stromatic Sphaeriales." *Am. Jour. of Bot.,* 13: 575–645, 1926.

[3] P. A. Saccardo, "Sylloge Fungorum." Vols. 1 to 24. Padua, 1882–1926.

[4] W. B. Grove, "New or Noteworthy Fungi, Pt. VI, *Phomopsis eres* Sacc." *Jour. of Bot.,* 56: 285–294, 1918.

Buisman[5] described a species of Phomopsis causg a canker on elm trees in Holland. As a result f cultural comparisons with cultures loaned through e courtesy of Dr. Buisman it appears that the rganism isolated by her corresponds for the most art with that of the present study.

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A NEW BACTERIAL DISEASE OF PEARS

IN the course of an investigation on the green uorescent bacterial plant pathogenes, a very pathoenic organism belonging to this group was obtained om a plate given me by Mr. K. G. Parker who was the time isolating *Erwinia amylovora*. The isolaon was made from colonies with green pigments in e medium, appearing as a contamination. The resence of such green fluorescent organisms in isolaon work is not an unusual occurrence. Frequently ey are discarded as saprophytes, in some cases with ood reasons. Their presence is usually given no gnificance because of their ubiquitous nature in lants, seeds, water and soil.

There is practically no information as to the relaon these organisms have to economic plants. There , however, an increasing realization that some of em are plant pathogenes. The organism here deribed is such an example and has not heretofore een known.

SYMPTOMS OF THE DISEASE

The manifestation of blight on flowers, which turn rk brown and die, is very apparent. On matured aves, black round spots are produced, while on young aves similar spots have a yellow "halo" and the aves often become distorted. Black, almost circular ots are produced on young and mature fruits, but ore readily on the latter.

PATHOGENICITY

Artificial inoculations on fruits, flowers, leaves and ems of a hybrid pear, *Pyrus communis* × *serotina*, early established the pathogenic nature of the oranism. Cross inoculations revealed a wide range of osts. Infection was obtained on *Vigna sinensis*, *haseolus vulgaris*, *Purearia hirsuta*, *Vicia faba* and *yringa vulgaris*.

A study of the organism showed it to be a *Pseudonas*, but a comparison with the descriptions of the nown species of these bacterial plant pathogenes und on pear, *Pseudomonas barkeri* and *Pseudomonas ectarophila*, showed no identity. Its fluorescigenic character is readily observed in a medium consisting of 0.3 gm $MgSO_4$, 2 gm K_2HPO_4 and 3 gm asparagine per liter, adjusted to about pH 6.9. It is closely related to *Pseudomonas syringae*, *Pseudomonas vignae* and *Pseudomonas viridiflava*, but distinctly not identical to any of these species. The organism is named and briefly described.

Pseudomonas utiformica sp. nov.

Motile by one to two polar flagella; rods with rounded ends; 0.7–1.5 x 1.3–3.1 μ, occurring singly and in pairs; no spores; no capsules; gram-negative; not acid-fast; facultative anaerobe; green fluorescent; beef-extract agar colonies round or fimbriate, grayish white to slightly greenish; nutrient broth fairly turbid in 24 hours; gelatine liquefied; milk not curdled, alkaline; nitrate not reduced or weakly reduced; ammonia produced; H_2S and indol not produced; dextrose, galactose, levulose, mannose, arabinose, xylose, sucrose, raffinose, manitol, glycerol, and salicin are fermented; no fermentation in rhamnose, maltose, lactose; starch and cellulose not digested; growth in malic, citric, succinic, formic, and lactic acids; no growth in tartaric acid.

Pathogenic on: *Pyrus communis* × *serotina*, *Vigna sinensis*, *Phaseolus vulgaris*, *Pueraria hirsuta*, *Vicia faba*, *Beta vulgaris*, *Syringa vulgaris*, *Prunus avium*.

The genus *Pseudomonas* is used according to Bergey's definition and as proposed by Burkholder. If the S. A. B. committee's recommendation for plant pathogenes is adopted the genus should be *Phytomonas*.

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THE PRODUCTION OF MUCIFICATION OF THE VAGINAL EPITHELIUM OF RODENTS BY THE OESTROUS HORMONE

THE production of vaginal mucification by *corpus luteum* extracts which maintain pregnancy in overiectomized pregnant animals, as described in a recent article in SCIENCE by Harris and Newman,[1] is, we believe, not a test for progestin but a test for the small amount of oestrin which the extracts used by them undoubtedly contain. In 1928, one of us, (R. K. M.) with Hisaw and Weichert,[2] described the production of a similar reaction in the rat with *corpus luteum* extracts. Shortly after, in 1929, the other, (W. M. A.) with Corner,[3] described the produc-

[5] J. Westerdijk and C. Buisman, "De iepenziekte rapport over het onderzoek verricht op verzoek can de Nederndsch heidemaatschappij." pp. 56–62, 1929.

[1] R. G. Harris and D. M. Newman, "A Practical Test for Potency of Extracts of *Corpora Lutea*." SCIENCE, 74, 182, 1931.

[2] F. L. Hisaw, R. K. Meyer and C. K. Weichert, "Inhibition of Ovulation and Associated Histological Changes." *Proc. Soc. Ex. Biol. Med.*, 25, 754, 1928.

[3] G. W. Corner and W. M. Allen, "Physiology of the *Corpus Luteum*. II. Production of a Special Uterine Reaction (Progestational Proliferation) by Extracts of the *Corpus Luteum*." *Am. Jour. Physiol*, 88, 326, 1929.

tion of progestational proliferation in the castrated rabbit with extracts of *corpora lutea*.

During the past year we have been fortunate enough to have the opportunity of working jointly in the same laboratory on these reactions in an attempt to ascertain whether progestin produces mucification. While engaged in this task, Robson, 1931[4], reported the production of vaginal mucification in mice with progestin-containing extracts which had been treated with alkali. The details were not given but the fact that progestin is readily destroyed by alkali makes these experiments of Robson quite significant. Furthermore, he was able to produce the reaction with small amounts of oestrin prepared from follicle fluid, the dose used being one half that necessary to produce cornification.

Previously to the report of Robson we had been able to produce mucification in the rat with *corpus luteum* extracts made by extracting the fresh tissue with acid alcohol, boiling alcohol, or benzene, active material being obtained regardless of the solvent used. These extracts also always produced progestational proliferation in the adult castrated rabbit and therefore contained progestin. The extracts we had used were quite crude and probably contained oestrin, since no chemical procedure was used which would absolutely remove it. Similarly, none of those experimenters[1, 2, 4, 5, 6, 7] who have produced mucification by *corpus luteum* extracts have proved that the extracts used did not contain oestrin. On the contrary, analysis of the methods used by these workers makes it quite certain that they all did contain oestrin. These findings led us to carry out experiments designed to produce mucification with standardized oestrin-containing preparations.

When the article by Harris and Newman appeared we had been able to produce mucification in the guinea-pig and mouse by continued dosage with small amounts of Squibb's amniotin (an oestrogenic preparation from the amniotic liquor of cattle). This did not of course do more than indicate that oestrin might be the factor. Since then we have produced mucification in one adult guinea-pig, new-born guinea-pigs, mice, and rats with Parke, Davis and Company's theelin (crystalline oestrogenic preparation from the urine of pregnant women). Being producible by theelin, it seems fairly evident that the mucification is not due to progestin since in the preparation of theelin hot aqueous alkali is used, a procedure which destroys the activity of progestin. We feel, therefore, that the ability of theelin to produce mucification makes it reasonable to suppose that those who have produced mucification with *corpus luteum* extracts were obtaining a result due to the oestrin present and not to other specific hormones which they contained (progestin, relaxin).

The method we have used in general is to castrate adult rats, mice, and guinea-pigs in heat and to start injections with the oestrin preparation the next day and to continue for many days. The vaginal reaction was carefully studied by daily smears and by serial biopsies in the rats and guinea-pigs. The biopsy method was very satisfactory, in the guinea-pigs especially, and certainly facilitated greatly the progress of the work. In the guinea-pig 0.5 r.u. for about 25 days will bring about mucification equal to that seen early in the second month of pregnancy but not equal to that seen 24 hours *antepartum*. In the mouse mucification indistinguishable from that of even late pregnancy is produced by 0.04–0.06 r.u. per day for 8 days. Similarly in the rat good mucification has been obtained but the exact dosage has not been as carefully ascertained. We have also produced good mucification in new-born female guinea-pigs by giving 5.0 r.u. per day for 4 days. Mucification has also been produced in mice with oestrin made by extracting male urine with benzene, thus showing that oestrin made from a source other than the female organism is capable of producing the result.

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